In Celebration of Canadian Scientists

A Decade of Killam Laureates

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In Celebration of Canadian Scientists - A Decade of Killam Laureates

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The Killam Program of the Canada Council

The Canada Council's Killam Program is one of five Killam Trusts funded by the late Mrs. Dorothy J. Killam through bequests of her will. The Council was named as one of the legatees of Mrs. Killam's residuary estate, together with Dalhousie University, the Universities of Alberta and British Columbia, and the Montreal Neurological Institute. Subsequently, part of the fund bequeathed to the University of Alberta was transferred to the University of Calgary. The Killam Trusts are all governed by the stipulations of Mrs. Killam's will and are supervised by the Killam Trustees. In addition to the bequest, the Canada Council also administers a fund which was donated by Mrs. Killam anonymously, prior to her death.

As expressed in her will, Mrs. Killam's purpose in establishing the Killam Trusts was to "help in the building of Canada's future by encouraging advanced study". Mrs. Killam hoped "to increase the scientific and scholastic attainments of Canadians, to develop and expand the work of Canadian Universities, and to promote sympathetic understanding between Canadians and the peoples of other countries". Mrs. Killam stipulated that the net income from each Killam Trust should be used to provide fellowships and other grants, all to be known as Killam Scholarships, for advanced study in any discipline other than "the arts", as defined by the Canada Council Act. It was her stated desire that "those selected to receive Scholarships shall be likely to contribute to the advancement of learning or to win distinction in a profession".

The Canada Council's Killam awards are aimed at established scholars who have proven themselves to be among the best and most productive researchers in their respective disciplines. The Council currently offers two types of awards, both of which are awarded on a competitive basis by the Killam Selection Committee.

Killam Research Fellowships are offered in support of advanced research in any of the disciplines of the humanities, social sciences, natural sciences, health sciences and engineering, as well as interdisciplinary research. The Killam Fellowships allow established scholars to devote up to two years to full-time research. The Committee awards Research Fellowships solely on the basis of scholarly excellence, taking into account both the achievement of the applicants and the relative importance of their research proposals.

The Izaak Walton Killam Memorial Prizes reflect Mrs. Killam's interest in the sciences, medicine and engineering. The Killam Prizes are not intended to honour a particular achievement, but rather are given in recognition of distinguished career contributions to the advancement of knowledge in any of the disciplines within the natural sciences, health sciences, and engineering. In addition to having made a distinguished career contribution, a Killam Laureate must still be actively engaged in research. An individual cannot apply for a Killam Prize, but instead must be nominated for the honour by distinguished specialists in his or her respective field. With the Killam Prizes, the Killam Committee seeks to honour and encourage world-class achievement by Canadian scientists. Mindful of the fact that it is the laureates themselves who ultimately will define the stature of these awards, the Committee is careful to award the Killam Prizes only to individuals who have, through their personal achievements, enhanced Canada's reputation in the international scientific community.

This collection of biographies of Killam Prize Laureates is intended to celebrate the remarkable extent and diversity of Canadian scientific achievement. The accomplishments of the Laureates and the Killam Fellows over the years exemplify the highest standards of research excellence and stand as an enduring testament to the remarkable vision and generosity of Izaak Walton and Dorothy J. Killam. The Killams recognized instinctively that Canada's future lies in the development of our intellectual resources, and they contributed magnificiently to that end by establishing the Killam Trusts. The distinguished contributions of the Killam Prize Laureates attest eloquently, not only to the foresight of the Killams, but also to the high standing of Canadian researchers in the international research community.

Mel MacLeod
Head, Killam Program

Le Programme Killam du Conseil des Arts du Canada

Le Programme Killam du Conseil des Arts du Canada est une des cinq fiducies Killam dotées par Mme Dorothy J. Killam par voie de legs dans son testament. Le Conseil a été nommé un des légataires à titre universel de Mme Killam, de même que l'Université Dalhousie, les universités de l'Alberta et de la Colombie-Britannique, et l'Institut neurologique de Montréal. Par la suite, une partie des fonds légués à l'Université de l'Alberta a été transférée à l'Université de Calgary. Les fiducies Killam sont toutes régies par des dispositions du testament de Mme Killam, et sont contrôlées par les fiduciaires. Outre ce legs, le Conseil administre aussi un fonds qu'il a reçu en don à titre anonyme de Mme Killam avant son décès.

Objet des fiducies Killam

Comme Mme Killam le mentionne dans son testament, son but en établissant la fiducie Killam était de "contribuer à préparer l'avenir du Canada en encourageant les études avancées". Mme Killam espérait ainsi "accroître les réalisations des Canadiens dans les domaines scientifiques et universitaires, élargir le champ d'action des universités canadiennes, et promouvoir la compréhension et l'harmonie entre Canadiens et étrangers". Mme Killam a stipulé que le revenu net de chaque fiducie Killam devait servir à financer des bourses et autres subventions, toutes devant être connues sous le nom de bourses Killam, pour des études avancées dans une discipline autre que "les arts", selon la définition de la Loi sur le Conseil des Arts du Canada. Mme Killam a expressément demandé que "les bénéficiaires des bourses soient aptes à contribuer à l'avancement des connaissances ou à se distinguer dans une profession".

Les distinctions Killam du Conseil des Arts du Canada sont destinées aux spécialistes reconnus qui se classent parmi les meilleurs et les plus actifs chercheurs dans leurs disciplines respectives. Le Conseil offre actuellement deux types de distinctions, toutes deux décernées à l'issue de concours tenus par le Comité de selection Killam.

Les bourses de recheche Killam sont offertes à l'appui de la recherche supérieure dans l'une ou l'autre des disciplines des humanités, des sciences sociales, des sciences naturelles, des sciences de la santé, et du génie, de même que pour les études unissant des disciplines de ces secteurs. Les bourses Killam permettent à des spécialistes reconnus de consacrer jusqu'à concurrence de deux ans à la recherche à plein temps. Le Comité attribue les bourses uniquement en fonction de l'excellence sur le plan de la recherche, tenant compte à la fois des réalistions des candidats et de l'importance relative de leurs projets.

Les Prix commémoratifs Izaac Walton Killam marquent l'intérêt porté par Mme Killam aux sciences, à la médecine, et au génie. Ils ne sont pas rattachés à une réalisation particulière, mais rendent hommage à une carrière remarquable consacrée à l'avancement de la connaissance dans une discipline des sciences naturelles, des sciences de la santé, et du génie. En plus de s'être distingué dans sa profession, un lauréat Killam doît être activement engagé dans la recherche. Les chercheurs ne peuvent, d'eux-mêmes, présenter leur candidature; ils doivent être proposés par des spécialistes distingués de leur domaine. En décernant ses prix, le comité Killam veut honorer et encourager les travaux de classe internationale chez les scientifiques canadiens. N'oubliant pas que ce sont les lauréats eux-mêmes qui, en fin de compte, déterminent la valeur de ces prix, le Comité a soin de n'attribuer les Prix Killam qu'à des personnes qui, par leurs réalisations personnelles, ont rehaussé la réputation du Canada dans les milieux scientifiques internationaux.

Le présent recueil de notices biographiques des lauréats des Prix Killam vise à célébrer l'étendue et la diversité remarquable du travail scientifique canadien. Les réalisations des lauréats et des boursiers Killam au cours des années illustrent les normes les plus élevées d'excellence en recherche et témoignent de façon durable de la vision et de la générosité insignes d'Izaak et Dorothy Killam, qui ont reconnu d'instinct que l'avenir du Canada repose sur le développement de nos resources intellectuelles et qui ont contribué à cet objectif de façon princière en établissant les fiducies Killam. Les contributions distinguées des lauréats des Prix Killam attestent avec éloquence, non seulement de l'esprit de vision de M. et Mme Killam, mais aussi du rang élevé qu'occupent les chercheurs canadiens au sein de la communauté internationale de la recherche.

Mel MacLeod
Chef du programme Killam

The Killams

Izaak Walton Killam was born in modest circumstances in 1885 at Yarmouth, Nova Scotia. He began his business career at the age of eighteen as a junior clerk in the Yarmouth branch of the Union Bank of Halifax. He was soon transferred to the Bank's head office in Halifax, where his talent and reputation caught the attention of Max Aitken (later Lord Beaverbrook), who was in the process of setting up the Royal Securities Corporation. In 1904, Aitken persuaded Killam to join Royal Securities, which became the foundation of his remarkable financial career. By 1914, Killam had replaced Lord Beaverbrook as president of Royal Securities, and soon after, he acquired Beaverbrook's shares in the firm. For forty years, Killam served as president of Royal Securities. In that time, the company became one of Canada's most influential investment houses, and Izaak Walton Killam established himself as one of the outstanding figures in Canadian financial history.

Recognizing the great potential of Canada's forest and hydro industries and the public utilities services, Killam played an active and personal role in the organization and modernization of many companies in these fields. The various Royal Securities holdings included power companies in Alberta, Ontario, Nova Scotia and Newfoundland, as well as pulp and paper companies in British Columbia and Nova Scotia. He had other interests as well, such as Moirs Limited of Halifax, which became one of the best known chocolate companies in Canada after he had completely revamped it.

Notwithstanding his prodigious financial accomplishments, Izaak Walton Killam was a reserved man who eschewed publicity and was virtually unknown outside a small circle of close acquaintances. True to his namesake, he was an ardent salmon fisherman throughout his life, and he shared with his wife, Dorothy, a passion for baseball, especially the Brooklyn Dodgers. Characteristically, the substantial contributions he made to charities throughout his lifetime were always given anonymously.

Dorothy Killam, née Dorothy Brooks Johnston, was born in St. Louis, Missouri in 1899, of moderately affluent parents. She was well-schooled, athletic and widely travelled with a good knowledge of French and German. Unlike her husband, Dorothy Killam was an extrovert who loved company and people generally. After she and Mr. Killam were married in 1922, they lived in Montreal, the centre of the Killam financial empire.

Izaak Walton Killam died suddenly and unexpectedly of heart failure in 1955 at his Quebec fishing lodge. He left all of his substantial estate to his wife, who subsequently demonstrated her own financial acumen by vastly increasing the fortune over the next ten years. When Mrs. Killam died in 1965, after several years of poor health, she left a will which gave effect to the plans she and her husband had discussed frequently in the later years of his life. While Mrs. Killam's will for the most part reflected the intentions of her husband, it also included a substantial bequest to build a children's hospital in Halifax as a memorial to him.

The Killam Trusts

Dorothy Killam bequeathed funds in trust to the following institutions:

- The Canada Council
- Dalhousie University
- The Montreal Neurological Institute
- The University of Alberta
- The University of British Columbia

In 1967, part of the bequest to the University of Alberta was transferred to the University of Calgary, when the latter institution obtained its own charter.

The Killam Trustees

- Donald N. Byers, Q.C.
- George T.H. Cooper, Q.C.
- John H. Matthews
- W. Robert Wyman

Les Killam

D'origine modeste, **Izaak Walton Killam** naît en 1885 à Yarmouth, en Nouvelle-Écosse, et entre à 18 ans dans le monde des affaires comme commis subalterne à la succursale de Yarmouth de la Union Bank of Halifax. Muté peu après au siège social de la banque à Halifax, son talent et sa réputation ne tardent pas à attirer l'attention de Max Aitken (futur Lord Beaverbrook), alors occupé à mettre sur pied la Royal Securities Corporation. En 1904, M. Aitken persuade I.W. Killam de se joindre au personnel de la Royal Securities, et c'est le début d'une brillante carrière financière. En 1914, M. Killam remplace Lord Beaverbrook à la présidence de la Royal Securities et, peu après, achète ses intérêts dans l'entreprise. Pendant quarante ans, il préside aux destinées de la société qui, sous son impulsion, devient l'une des maisons de placements les plus influentes du pays; lui-même s'impose comme l'une des figures marquantes de l'histoire financière du Canada.

Conscient de l'énorme potentiel des industries forestières et hydro-électriques du Canada et des services publics, M. Killam a activement et personnellement participé à l'organisation et à la modernisation de bon nombre de sociétés dans ces secteurs. Faisaient en effet partie du portefeuille de la Royal Securities des centrales hydro-électriques de l'Alberta, de l'Ontario, de la Nouvelle-Écosse et de Terre-Neuve, ainsi que des compagnies de pâtes et papier de la Colombie-Britannique et de la Nouvelle-Écosse. Parmi les autres sociétés dans lesquelles M. Killam avait des intérêts, Moirs Limited, de Halifax, est devenue, sous son influence, l'une des chocolateries les mieux connues du Canada.

Malgré ses prodigieuses réussites financières, M. Izaak Walton Killam était un homme très réservé. Il fuyait la publicité et était presque inconnu hors d'un cercle très restreint de relations personnelles. À l'instar de l'écrivain anglais Izaak Walton, à qui il doit son nom, il fut un infatigable pêcheur de saumon et, comme son épouse Dorothy, un adepte passionné de baseball, particulièrement des Dodgers de Brooklyn. Les dons généreux qu'il fit aux organismes de charité tout au long de sa vie furent toujours versés anonymement.

Dorothy Brook Johnston Killam est née en 1899 à St. Louis, au Missouri, de parents assez à l'aise. Sportive et grande voyageuse, elle avait également fait de solides études et possédait une bonne connaissance du français et de l'allemand. À la différence de son époux, elle était d'un naturel expansif et aimait la compagnie. Après leur mariage, en 1922, M. et Mme Killam vécurent à Montréal, centre de l'empire financier Killam.

Izaak Walton Killam est décédé subitement d'une crise cardiaque en 1955, à son chalet de pêche au Québec. Il laissait tous ses biens à son épouse qui fit preuve, elle aussi, d'une rare perspicacité financière en accroissant considérablement cette fortune au cours des dix années qui suivirent. Après plusieurs années de santé chancelante, elle mourut en 1965; son testament donnait effet aux plans qu'elle et son époux avaient arrêtés pendant les dernières années de leur vie commune. Il respectait en grande partie les intentions de M. Killam, mais comprenait en outre un important legs pour la construction, à sa mémoire, d'un hôpital pour enfants à Halifax.

Les fiducies Killam

Mme Killam a légué des fonds en fiducie aux établissements suivants:

- le Conseil des Arts du Canada
- l'Institut neurologique de Montréal
- l'Université Dalhousie
- l'Université de l'Alberta
- l'Université de la Colombie-Britannique

En 1967, une partie du legs destiné à l'Université de l'Alberta a été cédée à l'Université de Calgary, quand celle-ci obtenu sa propre charte.

Les fiduciaires Killam

- Donald N. Byers, C.R.
- George T.H. Cooper, C.R.
- John H. Matthews
- W. Robert Wyman

PREFACE

In Celebration of Canadian Scientists—A Decade of Killam Laureates is a bold attempt to both acknowledge and infuse excellence into our cultural ethos, which is presently restless and anxious for change in 1991—but in which direction? Furthermore, advances in science and technology almost overwhelm us with choices for change. Celebration in Canada is often perceived to be a peculiar and ambivalent activity and certainly not one to be indulged in by academics. And yet these twenty-five Killam Laureates, each blending a powerful mind with energy and human insight, are in themselves a splendid celebration of choice, challenge and excellence across Canada.

Like oceans, ideas can link or divide the continent. It is a question of attitudes. Throughout the world, science, engineering and medicine have increasingly shaped our lives and our perceptions of ourselves over the past century, and the past twenty-five years have witnessed extraordinary and exciting changes. These men and women, whose personal stories of excellence and achievement are recorded here, have contributed notably to the visions, the scientific breakthroughs, the innovative applications and the societal impacts surrounding their original ideas. Here the Killam Laureates have invisibly linked us all into their academic, national and international worlds, and we in turn must amplify the messages of excellence, pursuit, struggle and accomplishment that we read in each chapter.

The Killam Trusts since their inception have transformed the vision of Izaak Walton and Dorothy J. Killam into an intellectual treasure house for Canada. Over the decades an increasing number of Canada's finest scholars in all disciplines have joined that select family of Killam Fellows from whom, not suprisingly perhaps, many of the Killam Laureates have coincidentally and later emerged.

Excellence often begets excellence. It inspires both those in the immediate scholarly milieu and others, at a distance and over decades, through contact with journal articles, books, conference presentations, and distinguished public lectures.

Ideas and images are powerful drivers of cultural change. Thus the clear and witty communication of scientific results to a wider lay audience, through the print and electronic contemporary media, is of paramount importance. We must emphasize the shared roots of human creativity and resist deepening the artificial divide between arts and sciences in our culture. Science has its own intrinsic beauty and sense of wonder. There is a time and tempo to scientific discovery that is as human, and indeed as fragile, as crafting a novel or painting a winter landscape. The Killam Laureates in many ways exemplify this shared arts and sciences thrust of individual creativity, and their messages are for the wider audience.

This Book was inspired as a former Killam Fellow, a Killam Laureate, and the Head of the Killam Program met over lunch one day in August, 1988, to discuss how we could not only bring an even stronger focus on excellence and science to young people but do it through the Killam Trusts. As Chairman of the Science Council of Canada, I was particularly concerned about raising the profile of active scientists as role models for a generation increasingly unaware of the demands for scientific literacy in every citizen.

The Prime Minister and the National Advisory Board on Science and Technology had recently announced a new scholarship program. To be held at any Canadian university, in any area of Science and Engineering, the scholarships were open competitively to young Canadians in high schools across the country. I had been asked to address their inaugural banquet in the magnificent rooms of the West Block, at the Houses of Parliament, later in August. It was a magic warm Ottawa evening, full of the excitement and awe of the young prize winners, who mixed with the political names of the day from all parties with aplomb and with enthusiasm for the future. Yet the Killam Laureates who should have been heroes and heroines to these young Canadians were invisible compared to sports figures. On hearing some of the Killam stories, the young students were intrigued and surprised to know that these scientific leaders were all active in Canada!

These students, we decided at lunch, should be the first audience for our celebration of science. And that is how this Book began. From a modest beginning, the project rapidly took on a life of its own as we contacted the Laureates, and persuaded and cajoled them to write biographical chapters; we then became the voluntary editors of an anthology of excellence. The book grew, the audience expanded, the interest began to develop from outside scientific circles, and finally the text took shape as a celebration of scholarship and science in Canada and for Canada.

Three years later, much of an unexpected nature has occurred in Canada and extraordinary geopolitical changes have reshaped our Global Village, as we watch each day images of ourselves searching for a new pattern in history. Amid the uncertainty, however, one fact continues to offer hope and direction. Excellence sparkles. In striving for excellence, and in recognising that knowledge is the strongest international currency of all, we too can begin to share in the vision of the Killams by communicating and celebrating the stories of these Canadians who showed us all how to win with ideas.

Geraldine A. Kenney-Wallace
President and Vice-Chancellor, McMaster University
Past-president, Science Council of Canada

PRÉFACE

In Celebration of Canadian Scientists—A Decade of Killam Laureates constitue une tentative audacieuse de reconnaître et promouvoir l'excellence de notre génie culturel, lequel donne des signes d'agitation annonciateurs de changements en 1991; mais dans quelle direction? Les progrès scientifiques et technologiques nous offrent le choix entre de multiples avenues. Au Canada, les hommages rendus sont souvent jugés avec circonspection, surtout dans le milieu universitaire. Pourtant ces vingt-cinq lauréats Killam, qui tous allient intellect supérieur, énergie et humanisme, sont eux-mêmes un magnifique témoignage de l'excellence au Canada.

Comme les océans, les idées peuvent unir ou diviser les continents; c'est une question d'attitude. Partout dans le monde, la science, le génie et la médecine modifient notre vie et la perception que nous avons de nous-mêmes, et les derniers vingt-cinq ans ont connu des changements extraordinaires. Les hommes et les femmes dont ce livre décrit l'excellence et les réalisations ont contribué notablement à façonner les visions, les découvertes scientifiques, les applications innovatrices et les retombées de leurs idées originales pour la société. Ainsi les lauréats Killam nous ont fait participer à leur monde universitaire, national et international, et à notre tour, nous devons faire retentir les messages d'excellence, de motivation, de lutte et d'accomplissement que nous lirons dans les chapitres qui suivent.

Depuis leur création, les fiducies Killam ont transformé la vision d'Izaak Walton et Dorothy J. Killam en un trésor intellectuel pour le Canada. Au cours des années, un nombre croissant d'éminents chercheurs du Canada, de toutes disciplines, se sont joints à la famille distinguée des boursiers Killam dont bon nombre des lauréats Killam sont issus.

L'excellence engendre l'excellence. Elle inspire les collègues immédiats et tous ceux qui, éloignés par la distance et par le temps, y sont exposés par l'intermédiaire des articles de revues, des livres, des communications et des conférences.

Les idées et les images sont de puissants agents du changement culturel, d'où l'importance primordiale de la diffusion des résultats scientifiques auprès du grand public par les médias contemporains, écrits ou électroniques. Nous devons reconnaître les racines communes de la créativité humaine et éviter la division artificielle entre les arts et la science dans notre culture. La science a sa propre beauté intrinsèque et son propre merveilleux. La découverte scientifique est un travail aussi humain et fragile que la création d'un roman ou d'une peinture. De bien des façons, les lauréats Killam reflètent cette union de l'art et de la science dans la créativité individuelle, et leur message s'adresse aussi au grand public.

Le présent ouvrage a pour instigateurs un ancien boursier Killam, un lauréat Killam et le chef du Programme Killam, qui se sont rencontrés au déjeuner un jour d'août 1988 pour discuter des moyens de faire davantage connaître l'excellence et la science aux jeunes, par l'intermédiaire des fiducies Killam. À titre de présidente du Conseil des sciences du Canada, j'étais particulièrement désireuse de faire ressortir plus clairement le rôle de modèles que peuvent jouer les scientifiques auprès d'une génération de moins en moins consciente de la nécessité pour tous de comprendre les phénomènes scientifiques.

Le premier ministre et le Conseil consultatif national des sciences et de la technologie venaient d'annoncer un nouveau programme de bourses. Ces bourses, offertes à de jeunes élèves canadiens des écoles secondaires du pays, leur permettraient de faire des études en science ou en génie dans une université canadienne. On m'avait invitée à prendre la parole au banquet inaugural qui s'est tenu dans les magnifiques salles de l'Édifice de l'ouest du parlement. C'était une douce soirée d'août, pleine de magie et de ravissement pour les jeunes boursiers qui ont pu bavarder avec les personnalités politiques du jour, avec aplomb et enthousiasme. Et pourtant, les lauréats Killam, qui auraient dû être les héros et héroines de ces jeunes canadiens, étaient inconnus comparés aux grandes vedettes du sport. En entendant parler des lauréats Killam, les étudiants furent intrigués et étonnés d'apprendre que ces grands scientifiques travaillaient tous au Canada!

Ces étudiants, nous avons décidé, devaient être les premiers à partager notre hommage à la science. Et c'est ainsi qu'a pris naissance ce livre qui, d'un modeste projet, a vite grandi quand nous avons contacté les lauréats Killam pour les convaincre de rédiger chacun un chapitre biographique; puis nous nous sommes retrouvés directeurs, bien consentants, d'une anthologie de l'excellence. Le livre pris de l'ampleur, l'audience également; ce serait dorénavant une hommage aux recherches et aux travaux scientifiques au Canada et pour le Canada.

Trois ans plus tard, le Canada a connu toutes sortes de changements imprévus et notre village global a lui-même dû se soumettre à d'extraordinaires modifications politiques. Dans ce climat d'incertitude, toutefois, une chose continue à offrir espoir et à guider: l'étoile de l'excellence. En poursuivant l'excellence, et en reconnaissant que le savoir dépasse les frontières internationaux, nous pouvons partager le rêve des Killam en célébrant l'histoire de ces grands canadiens qui nous ont montré la puissance des idées.

Geraldine A. Kenney-Wallace
Présidente et Vice-Chancelier, Université McMaster
Ancienne présidente, Conseil des sciences du Canada

FEROZE NOVROJI GHADIALLY

(November 13, 1920–)

Killam Laureate, 1981

Feroze Novroji Ghadially was born in Bombay, India, of Zoroastrian parents but, for reasons difficult to fathom, his birth was not registered. His father, Novroji Ghadially, was a well-to-do entrepreneur, mechanical engineer, and inventor with several patents to his credit. In his workshop, which employed over a hundred workers, locomotive parts, which he had invented, were manufactured. His mother, Testarbanoo Ghadially, was a part-time commercial artist who specialized in painting and decorating sarees. His sister, Monie Taraporvalla is now a retired Professor of English Literature (Bombay University) and a virtuoso pianist whose concerts have been broadcast in India and Australia.

When Ghadially was 4 years old, his father died. He was reared by a mother who was fanatical about educating her children and a wise kind grandmother who had been a school teacher all her life. Ghadially's early school days at the St. Xavier's School were traumatic, largely because he refused to accept, without questioning, the printed word, religious dogma, and the rules and regulations which he considered were silly. When virtually daily caning failed to reform this obstinate temperamental rebel, his mother was asked to remove him from school.

Before he could get into trouble in the next school (Masters High School), an event occured which was to transform his life—he encountered the textbook of Physics and Chemistry by Godfrey and Hodges. He still remembers the excitement with which he read and reread the book and tried to do as many experiments as possible at home, including the successful making of gunpowder! The virtually overnight transformation of an unremarkable student to one who stood first in science raised suspicions of cheating, and led to canings to confess the "truth". However, investigations soon revealed that Ghadially had exceptional powers of memory and recall which permitted him to memorize and recite entire textbooks as if they were little poems. Now labelled a "parrot" and a "freak", he made few friends and retreated into the public library where he read innumerable books and magazines, not only on scientific topics but on just about any and every subject imaginable.

This large scale extracurricular activity resulted in his failing the matriculation examination of Bombay University in 1935, but he passed this examination in 1936. By this time he had made up his mind to be a scientist, and, when asked at the interview for admission into the premedical course "Why do you want to be a doctor?", he replied: "I don't want to be a doctor. I am going to be a medical scientist". However, at this stage he had fallen in love with classical music and had begun to study violin playing under Jules Crane, a gold medallist of the Paris

Conservatoire. Now he was not sure whether he wanted to be a virtuoso violinist or a scientist, but 15 years later (in London, England), after studying violin playing with Eric Harvey at the Trinity College of Music, London, he abruptly abandoned violin playing and took up sculpting in wood, metal, and stone. Ghadially has won awards for artistic achievements in mosaics and sculpture. His stained glass window based on motives derived from the art of the Haida Indians was included in a touring exhibition of modern stained glass in England. In Canada, his continuing fascination with Haida Indian art finds expression in the marquetry panels he produces.

While in premedical and medical school he developed several further interests, the principal ones being: (1) electronics and radio communications, specializing in the construction of Hi-Fi amplifiers, and remote control systems; (2) Leica (35mm) photography, specializing in the production of large exhibition prints and close-up photography of small objects and biological specimens; (3) making Newtonian telescopes; (4) writing articles, one of which, entitled "The Electron Microscope", earned him the Grant Medical College Magazine Prize (1941) for the best article of the year. Little did he or anyone else realize what a major role this instrument was to play in his career or that one day he would be regarded as one of the leading diagnostic electron microscopists in the world.

Once again Ghadially's extracurricular activities created problems and his clinical teachers considered him to be an undesirable student. However, his budding talents were recognized by Dr. D. M. Telang, Professor of Physiology at the Grant Medical College, Bombay. A close friendship developed between the two because Ghadially could perform experiments in the Physiology laboratory with a speed and accuracy beyond compare and could repair laboratory equipment which no other person could. Soon his reputation spread and he developed quite a lucrative business repairing scientific instruments and manufacturing incubators, ovens, and distillation apparatus.

Little wonder then that Ghadially failed in his final M.S., B.S. examination; ironically, one of the subjects he failed in was Pathology! When he did at last condescend to open Boyd's Textbook of Pathology, he felt the same excitement he had felt when he had opened his first science textbook. He now knew that he was going to be an experimental pathologist. Thus was born the career of a pathologist regarded by his peers as a "pathologists's pathologist"; "a world authority on Ultrastructural Pathology"; and "one of the foremost diagnostic electron microscopists in the world". His book, "Ultrastructural Pathology of the Cell and Matrix", is referred to as "the Bible", and the EM division he created in Saskatoon became the "Mecca of Ultrastructural Pathology" where students from all over the world came to attend his annual tutorials on diagnostic EM.

After obtaining the M. B., B. S. (Bombay) in 1944, Ghadially worked for six months as a demonstrator in Pathology at the Grant Medical College. To pursue his dreams of becoming an experimental pathologist, he sailed on a troop ship to

London, England, in 1945 where he had obtained admission to the London Hospital, Whitechapel, London, thanks to one of the most illustrious and lovable Deans of Medicine, A. E. Clark-Kennedy, renowned among other things for his ability to circumvent rules and cut through red tape. What impressed Clark-Kennedy were the long telegrams in which Ghadially explained his ambitions and dilemma. This direct admission to the Medical school circumventing "normal" channels raised the ire of some bureaucratic agencies, but Clark-Kennedy and Ghadially weathered the storm and a warm relationship developed between them; as the years went by, Dean Clark-Kennedy realized that he had intuitively selected a talented student.

Ghadially's M. B., B. S. (Bombay) was not recognized by the London University; so he was asked to attend classes and repeat all exams starting with the first year subjects, such as anatomy, physiology, and biochemistry. However, the M. B., B. S. (Bombay) was recognized by the Grand Medical Council of Great Britain. Thus developed a comic-opera-like situation whereby a medical student was also a fully qualified physician who did many locums to earn money to pay for his musical studies and attend concerts. (The more prosaic expenses were covered by a scholarship.)

At this time Ghadially had no intentions of obtaining a full-time job or getting married and settling down lest it interfere with his goal to be a scientist. Therefore, he obtained a non-paying appointment at St. Thomas's Hospital, London, in the laboratory of Dr. Joseph Bamford, Director of the Louis Jenner Laboratory, where he worked on a project which led to the production of a thesis entitled: "Changes in the blood and haemopoietic tissues produced by adrenaline" for which he was awarded the M. D. (London) in Morbid Anatomy and Histology in 1949. The literature searches, preparing of graphs, charts, and tables, and typing were executed by Miss Edna May Bryant, who worked in the A. G. Stats department of the War Office in London. A romantic attachment soon developed between them and they were married in August, 1950. No event has had a greater impact on his work than his marriage, for his indefatigable wife totally banned him from doing any household chores or worrying about money or budgets. On top of this, she has found time to act as his secretary and literary assistant in producing his many papers and books.

When attempts to obtain a position in an academic department of pathology in London failed, he applied for such a job at the University of Sheffield (1950). At the interview that followed, members of the committee were not impressed and were mildly amused; when asked the usual silly question "why do you want this job?", Ghadially replied "I know I am going to be a great scientist one day; so I need a laboratory to work in". Fortunately, Professor Harry Green, head of the department of pathology and Director of the Medical Research Council Traumatic Shock unit, was impressed. He took Ghadially on a tour of his laboratories and said "I will make you an assistant lecturer in my department and give you all the equipment and facilities you need; let us see what you are made of".

Thus, after long wanderings, the research career of Ghadially at last began; a year later (1951) Green and Ghadially published a paper entitled "Relation of shock, carbohydrate utilization and cortisone to mitotic activity in the epidermis of the adult male mouse" which is quoted in the literature to this day. Several other papers led to the thesis entitled "Effect of the adrenal hormones on mitotic activity" for which he was awarded a Ph. D. by the London University in 1955. He also started work on hair growth cycles and cutaneous carcinogenesis which was to earn him much recognition in later years. However, this happy interlude soon ended, for excessive dedication to science and little interest in service and committee work incurred the displeasure of Green's colleagues. Professor Green therefore moved to take up the position of Director of Cancer Research at the University of Leeds. Research in Pathology departments is complicated by the fact that most staff members do routine service work and have neither the time nor energy left for research. Such workers (referred to as "meat men" because they do autopsies) tend to resent experimental pathologists (referred to as "rat pathologists") and complain that, while they do all the real work, these rat pathologists get all the fame, glory, and free trips to far-off places.

Professor Harry Green was replaced by Professor D. H. Collins with a mandate to "normalize" the department of pathology. His main claim to fame lay on a small but excellent book entitled "Pathology of Articular and Spinal Diseases" which was based on his autopsy and histological observations. He made Ghadially to do his "fair share" of service work and did several things (for example, withholding salary increases and promotion) to encourage Ghadially to move. But for the protection offered by his tenured positon as a lecturer in pathology, he may well have lost his job. Ghadially could not possibly move because he had several long term experiments on chemical carcinogenesis on the go; these involved almost a thousand experimental animals (rats, mice, rabbits, guinea pigs, hamsters, coypus (Myocastor coypus Molina), ferrets, and hedgehogs).

However, Ghadially's studies on hair growth cycles (started in 1951 with Dr. H. J. Whitely before he left Sheffield) and his studies on cutaneous carcinogens started when Professor Green was department head, were now beginning to fructify. This work was aimed at determining the histogenesis of cutaneous tumours in man and similar tumours produced by the application of chemical carcinogens to the skin of animals. Ghadially succeeded in elucidating the histogenesis and biological behaviour of keratoacanthomas and he also discovered the melanocytic networks in the skin of the hamster from which melanomas arise in this species. This work (reported in several papers published from 1952 to 1965) has now been confirmed by numerous other workers: it is widely recognized and has been quoted in numerous papers and standard texts on skin diseases, tumours, and pathology (for example, Boyd, Willis, Bellisario, Lever, Armed Forces Atlas of Pathology, etc.). The work has been reviewed by Ghadially in all editions of "Dermatology in General Medicine", edited by T. B. Fitzpatrick et al. , and "Pathology of Tumours in Laboratory Animals, Vol. III, The Hamster", edited by V. Turusov, which was published by the World Health Organization. The former review has been called a "mini-classic" and Ghadially has been called the "Godfather of experimental keratoacanthoma".

However, the practical value of this work must not be forgotten. In the past, keratoacanthoma (a benign tumour) was mistakenly diagnosed as squamous cell carcinoma; at times, unnecessarily drastic surgical procedures such as amputation of a limb have been employed to treat this condition. During the active growth phase this tumour so closely resembles a squamous cell carcinoma that it has been used to illustrate "typical squamous cell carcinoma" in textbooks; indeed, many student classes in histopathology have been taught the microscopic morphology of squamous cell carcinoma from sections of keratoacanthoma. Ghadially's work and that of others has now clarified this diagnostic dilemma and led to the more rational treatment of patients. In recognition of his many scientific contributions, London University conferred upon him the D.Sc. in Physiology in 1962. In 1966, he was promoted to the rank of Senior Lecturer in Experimental Pathology at the University of Sheffield, but the accompanying title "Consultant in Pathology" was denied him.

Despite the pressure of work at Sheffield University, Ghadially found time to build a greenhouse where he cultivated orchids and a fish house where he bred thousands of tropical fish in over a hundred large fish tanks connected by a circulating range system which he had devised. He also served as President of several Aquarist Societies and travelled widely to lecture on this subject. Further, he authored 54 articles and 4 books on tropical fish which have been reprinted several times. His review article on White Spot disease has proven of practical value to amateur and professional pisciculturists. It was reprinted in the Aquarist in 1964 with an Editorial comment which reads "when we decided that we would publish a review article on White Spot disease to meet the demand from many readers, it soon became apparent that the information presented by Dr. F. N. Ghadially eight years ago could not be improved upon. Nothing required to be added or amended....".

The combination of his interest in fish and photography led to the production of several cine films. One of these, entitled "Breeding the Brown Acara", received a three star award at the amateur cine world contest in 1954, and sequences from the film were televised by the British Broadcasting Corporation.

In 1963 a patient suffering from carcinoma of the breast died and left a substantial sum of money to Sheffield University for cancer research. With the money allocated to the Department of Pathology, an electron microscope was purchased more as a matter of departmental prestige than for any other reason. Nobody, including Ghadially, wanted the instrument or was prepared to spend the time and effort needed to evolve the complex technology associated with electron microscopy. However, when the microscope was installed, Ghadially could not resist playing with it to see what it could do.

The very first specimen examined was the liver of one of his experimental rats which bore a carcinogen-induced sarcoma in the flank. As beginners luck would have it, he made the interesting discovery that there was a massive increase in hepatocellular lysosomes. With Dr. E. W. Parry, his newly appointed post graduate student working under him for the M. D. degree, he published his first ultrastructural study in 1965 entitled "ultrastructure of the liver of the tumour bearing host". The highly research-motivated Parry investigated this phenomenon in detail and produced a thesis for which he received the M. D. degree with honours. This work soon attracted international attention, and a large literature (biochemical studies and electron microscopy) has grown around their work on the liver of the tumour bearing host.

Relations between Collins and Ghadially had become reasonably cordial now. He had changed his opinion of Ghadially from "he knows less pathology than a medical student" to "he is a talented pathologist who will go far". Therefore Ghadially agreed to look at articular cartilage (Collins's speciality) with the electron microscope. Studies by Ghadially, Collins, and his research associate Dr. G. Meachim led to the production of papers on the lipid content of articular cartilage which are considered classics and quoted in virtually every review and book on articular cartilage to this day. Ghadially was the first to describe and identify correctly the origin of membrane-bound vesicles seen in the matrix of articular cartilage some four or five years before they were described in epiphyseal growth cartilage and their role in enchondrial ossification realized. A truly massive literature now exists on this subject.

In 1964, Dr. Subimal Roy (from Delhi, India) came to Sheffield University to work under Collins for his Ph. D. degree, but when he arrived he found that Collins had died of a heart attack. Ghadially did not want to accept Roy (supported by the Arthritis and Rheumatism Council of Great Britian) as his Ph. D. student because Ghadially had little knowledge of, or interest in, articular diseases. However, after much persuasion by Parry, who had developed an instant friendship with Roy, Ghadially agreed to accept him as his Ph. D. student.

Roy proved to be an intelligent and dedicated worker who not only produced a successful Ph. D. thesis in record time but also collaborated with Ghadially in producing 13 papers and a small book (185 pages) entitled "Ultrastructure of Synovial Joints in Health and Disease". Their experimental studies and studies on human pathological articular tissues are frequently quoted in the literature to this day. These include their experimental studies on haemarthrosis and lipohaemarthrosis, demonstration of hyaluronic acid secretion by synoviocytes, and pathogenesis of rheumatoid arthritis.

Professor Collins was replaced by Professor W.A.J. Crane, a committee man reputed to be a good organizer and adminstrator, but whose research record was unremarkable. He was envious of Ghadially's reputation as a scientist and was unable to live with Ghadially's title of "Senior Lecturer in Experimental Pathology". To change this title, he promoted Ghadially and labelled him "Reader in Neoplastic Diseases"; this was at a time when Ghadially was publishing more on joint diseases than tumours. Professor Crane's attempts to organize and administer research resulted in the departure of Drs. Parry and Meachim to Liverpool University, Dr. Roy to the All-India Institute of Medical Sciences, and Ghadially to Saskatoon, Canada. Crane died a few years later from a heart attack.

Ghadially came to the University of Saskatchewan in November, 1968, with a mandate to develop and direct a laboratory for electron microscopy within the department of Pathology. (He has since become a Canadian citizen.) Because of his dual interest in the Arts and Science, he had come to sharply distinguish technique from creativity. Having had some experience with sophisticated "state of the art" electron microscopes, he had come to the conclusion that such instruments were veritable white elephants which can hamper creativity and productivity. He had at this point in his career decided to study a vast variety of normal and pathological tissues and produce a book where just about everything that had ever been seen in the cell with the electron microscope would be catalogued, classified, illustrated, and explained.

When he came to Canada, he had the chance to put his philosophy about technique and creativity into practice. He chose to buy a simple basic electron microscope with a resolving power of only 1 nanometer which cost a modest $29,000, rather than a "state of the art" instrument with a 0.2 nanometer resolution priced at about $80,000 in those days. This was despite the fact that monies were available from the Medical Research Council of Canada to purchase any instrument of his choice. Needless to say, his choice caused much consternation among colleagues who could not understand how anybody in his right mind would actually prefer to buy a "cheap microscope". He argued that, since the specimens used in pathology research set the limit of resolution to about 2 nanometers, there would be no advantage gained by using a microscope capable of extremely high resolution; indeed, numerous disadvantages would follow because such instruments are inherently slow to operate and are exceedingly demanding in maintenance and day-to-day alignment to keep them operational.

Thus, by curtailing senseless technical demands, he was able to enhance his productivity many times. And some 6 years and 40,000 electron micrographs later, his book entitled "Ultrastructural Pathology of the Cell" was published in 1975. Two known competitors, one in the USA and another in Germany, working with elaborate "state of the art" microscopes and an entourage of co-workers and technicians, abandoned their attempts to produce a similar treatise when his book was published. The third edition of this book now entitled "Ultrastructural Pathology of the Cell and Matrix" (1400 pages, two volumes) was published in November, 1988.

Around 1977 it became evident that the basic knowledge that had accrued in the field of ultrastructural pathology, thanks to Ghadially and other workers, could be put to practical use in the diagnosis of various diseases, particularly tumours. Because of Ghadially's interest in basic curiosity-oriented research rather than applied research, he was a bit slow to accept this, and adhered to the popular idea of the time that the electron microscope was a research tool and research tool only. When finally the realization dawned, he found once again that two groups of workers, one in the USA and another in Australia, had been working for about a year or two on books dealing with diagnostic electron microscopy. Nevertheless, he joined the race and produced in less than 6 months his book entitled "Diagnostic Electron Microscopy of Tumours" about 1 year and 2 years before the competition. The first printing of this book sold out within 4 months of publication, and it was listed as one of the top six medical books published in 1983 by the Medical Writers Group of the Society of Authors, London, U. K. A much enlarged (500 pages) second edition of this work was published in 1985.

Ghadially does not wish to pretend that his views on technique and creativity engendered by his artistic interests and expressed in the choice of an electron microscope were wholly or solely responsible for the events that followed. However, he does contend that this was a major factor, and that jettisoning superfluous technique enhanced perceptiveness and the speed at which original ideas were pursued.

All this does not mean that Ghadially is blind to new technical developments or late in adopting them. He was one of the first to embrace the new technique of electron-probe x-ray analysis and he is now considered a pioneer in this field. More papers about electron-probe x-ray analysis of pathological tissues have come out of his laboratory than any other in Canada. His expertise in this field is widely acknowledged as evidenced by the several invited reviews that he has written and invitations to lecture at International Meetings. His main interest is in tracking and localizing metals (for example, gold, platinum, uranium, and bismuth) in cells by electron-probe x-ray analysis with a view to elucidating the manner in which they produce their therapeutic and toxic effects.

Besides the books mentioned above, Ghadially has produced two others: "Diagnostic Ultrastructural Pathology. A self-evaluation manual" (120 pages), which was published in 1984, and "Fine structure of Synovial Joints" (348 pages), published in 1983. He has also authored or co-authored 218 papers, reviews, and chapters in books. The quality or impact of his work is evidenced by the fact that he has now a science citation rating of 225 (the average citation rating per cited author is less than 5). For his many and varied achievements, Ghadially received an Izaak Walton Killam Memorial Prize in 1981 and an honorary D.Sc. from the University of Guelph in 1986. He is currently a Fellow of the Royal Society of Arts, London, U. K.; Fellow of the Royal College of Pathologists, U. K.; and Fellow of the Royal College of Physicians and Surgeons of Canada.

Ghadially is not enamoured with the referee systems operated by journals and granting agencies. He recalls (as do several distinguished workers, including Nobel laureates) that virtually all of his truly original studies arguing against established dogma were rejected by at least one and sometimes several journals. An example of this is his ultrastructural study with Parry on melanosis coli where they showed that the pigment was lipofuscin and not melanin and where they describe a new structure now called a "rod-body". Their paper on this subject was rejected by 5 journals (over a period of 2 years) and ultimately published in the Journal of Pathology and Bacteriology (1966) after much bickering. The paper is frequently cited to this day and, in a paper on melanosis coli published in 1988 by Walker, Bennett and Axelsen, it is proposed that the condition be renamed "lipofuscinosis coli" as suggested by Ghadially in 1975.

It is the experience of Ghadially and some of his illustrious colleagues (for example, Dr. Louis Jaques, F.R.S., of heparin fame) that increasing fame and international recognition brings in its wake the envy of at least some members on a grant committee and causes difficulties in raising grants. To circumvent this dilemma, Ghadially suggests that citation ratings, which represent objective international peer assessment of quality and impact of work, be employed to fund proven good workers. His proposal is that scientists who are publishing actively and who have a citation rating of say over one hundred (that is, 20 times greater than the average citation rate of 5 or less) be automatically funded at a certain basic level, and that the time of these precious few should not be wasted in making grant applications (junior workers and others could be funded via the usual referee system which, though defective, is the only one we have). He estimates that implementation of such a scheme would result in a 10 to 20 percent increase in the productivity of this elite group of scientists.

Besides being an internationally renowed scientist and scholar, Ghadially is also a popular lecturer who is invited again and again to lecture at Universities and meetings all over the world. He is regarded as a charismatic lecturer (orator according to some) who can hold audiences enthralled for hours on end. For example, at his annual tutorial he lectures for 5 hours a day for 4 successive days. At several meetings he has lectured at in recent years, he has been awarded medals and prizes.

Despite his achievements, which have brought international recognition to the University of Saskatchewan, and despite supporting letters from his veterinary and medical colleagues, including his department head, Dr. H. E. Emson, Ghadially was recently forced into retirement by the administrators of the University of Saskatchewan because he had reached the mandatory retirement age. He was reemployed as an "out of scope" employee, with a salary less than any of his technicians, to continue his work. Soon after, his signing privileges on a vital research account needed for the continuation of his work and assisting that of others, were transferred to his department head (a renowned organizer, administrator, and committee man), who told Ghadially what he must or must not do and outlined new rules and regulations under which he must henceforth operate. Since begging for funds, budgeting and justifying expenses is not his forte, he resigned and moved to Ottawa, largely because he thinks it is the most beautiful city in Canada. A few months after moving to Ottawa he was appointed Adjunct Professor of Pathology at the University of Ottawa. He now works in the Pathology Department of the Ottawa Civic Hospital and the renowned Canadian Reference Centre for Cancer Pathology. The difficult-to-diagnose cases referred to the experts who work here from hospitals all over Canada and abroad provide unique material for his ongoing work on diagnostic ultrastructural markers.

Thus more by accident than design Ghadially finds himself in an intellectual haven populated by dedicated colleagues interested in electron microscopy and various other new techniques which help to arrive at a more accurate diagnosis. In recognition of Dr. Ghadially's outstanding scientific contributions, the Society for Ultrastructural pathology will be holding a week-long (July 29–August 4, 1990) Conference in Diagnostic Electron Microscopy in his honour in Utah, U.S.A.

RAYMOND URGEL LEMIEUX

(June 16, 1920 –)

Killam Laureate, 1981

> "… mon seul but, pendant toute ma carrière, et choisi consciemment, a toujours été d'essayer de faire mon travail au mieux de mes facultés et toujours dans un esprit d'amélioration pour préserver autant que possible ma dignité personnelle."[1]

I. Introduction

Chemistry was my favorite subject in high school. Otherwise, my becoming involved in Chemistry was the result of a series of accidents leading in 1943 to graduation from the University of Alberta in honors Chemistry. By then, my mind was so conditioned by my favorite professor, Reuben B. Sandin, that I felt an urge for graduate studies and involvement in research as a career.

When I graduated with the Ph.D. degree from McGill University in 1946, I had abandoned all other thoughts for earning a living. It was clear that this decision could mean sacrifices in time devoted to family life and could result in a rather narrow life-style. On the other hand, success in research could be compensated by the "thrill of discovery" and what this brings in terms of recognition (and compensation). It was a difficult decision to make.

After some hesitation, I decided on an academic career, primarily because this brought the opportunity to do the kind of research where it can be hoped that discoveries along the way will prove more important than the initial goals set for the research program (Note 1). Although the setting of a research plan to better explore already discernible features of a body of knowledge is not without interest, I rather doubt that such activity alone would have kept me in research for a lifetime. As it turned out, it was the unexpected rewards in terms of true discovery that cropped up from time to time that maintained the necessary level of enthusiasm for the highly demanding business of competitive research at the cutting edge of knowledge.

Throughout nearly forty years of research conducted in both government and university laboratories, I functioned largely on major operational grants for the building of research groups in areas of my choosing. This was important since my interest in chemical research derived mainly from the excitement of discovery and, under the leadership of President E.W.R. Steacie of the National Research Council, the opportunity to do so existed in Canada at that time. Of course, life is never idyllic: engagement in research toward commercial ends could not be entirely avoided even had I wished it to be. With few exceptions, I managed to keep such activity within the corporate sector and divorced from my university laboratory. In fact, I am very proud of a number of industrial developments which occurred as the result of my involvement as a research chemist.

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[1] Saul Wolfe, *Some Thoughts as Ray Lemieux Turns 65*, Canadian Chemical News, June 1985, p. 25.

II. Education

My first encounter with the "thrill of discovery" in science was when still in high school. It proved not to be a discovery at all but it did provide insight into what a scientific career might mean to a poor motherless boy from the toughest part of Edmonton. This happened one day while examining a text book on physical chemistry at the public library. While trying to learn the difference between heat capacity and specific heat, I came to realize that there existed a rather linear relationship between the specific heats and the atomic weights of the elements. I brought this to the attention of my teacher, but he had little time for items not prescribed for his course.

Just at that time, my sister Annette was being courted by a graduate student in physics, John Convey, who showed a real interest in my high school studies and provided me with much guidance, especially in mathematics. I asked John if he knew that the product of the specific heat and the atomic weight of an element is 6.4. It was a bit discouraging to learn that I had rediscovered the Dulong-Petit law which dates back to 1819. Nevertheless, the seed for my career as a chemist was really planted then. It was John who acquainted me with the possibility of actually earning a living as a research chemist, and it was his bride-to-be who paid a large part of the tuition fee for my first year at the university (Note 2).

I had had absolutely no preparation for the excitement that chemistry courses would bring—especially in my first year at the University of Alberta. I took to organic chemistry "as a duck takes to water" and, at the start of my senior year, I asked my beloved professor, Rube Sandin, to allow me to prepare my B.Sc. thesis under his direction. When he asked why, I replied that I wished to be an organic chemist. "Ray", he said, "you should first try to be a chemist. Go work with Jack Morrison; he can use some help on a project related to detonators."

III. The Early Years of Research

A. *From charcoal to antibodies with personal and configurational relationships in between*

The project on detonators with Jack Morrison did not amount to much. Nevertheless, after my graduation in the spring of 1943, I stayed on with Jack for the summer in order to examine what happens to coconut charcoal in the course of its activation for use in gas masks. Neither Jack nor I had any idea of what to do with the several samples at different degrees of activation that had been received, and there were no directives at all from the Office of Chemical Warfare and Smokes in Ottawa. The only thing to do was to go to the library.

I read throughout May and then June. Jack Morrison was getting very nervous but he had no better idea than I did—so, I kept reading into July. With about six weeks to go, I came to the decision that perhaps the most practical thing to do would be to examine the adsorption of lower fatty acids from water by the charcoal. This approach was practical since I had worked the previous summer in a cordite manufacturing plant near Winnipeg, and had become highly proficient in volumetric analysis. Of course, I had learned from the literature that the acids are well adsorbed by charcoal.

I completed the project in one month and wrote the draft for a paper just in time to go to Montreal, loaded with sandwiches for a three-day trip to register for graduate studies at McGill University under the supervision of Clifford B. Purves, at the Pulp and Paper Research Institute of Canada. I am still proud of the results obtained on charcoal where it was found that the activation reached a maximum and then deteriorated because the collapse of the partitions between the pores led to larger pores but smaller surface areas.

At McGill, research related to the war effort continued, first on oxycellulose and then nitrocelluloses. I wrote a thesis entitled, "Hindrance Effects in Cellulose Substitution Reactions". The work was not particularly engaging but the association with Professor Clifford B. Purves put the final touch to my decision to seek a career in research, especially in the general area of carbohydrate chemistry. It appeared to me that glucose was a most fascinating structure and the queen of all natural products. Also, carbohydrates, in general, were very intimately and broadly involved in living processes which really were the main source of my interest in studying chemistry as a career. It was in Dr. Purves' office (he never came to my laboratory) in private discussions, where we smoked many of his "tailor-made" cigarettes, that I became completely entranced with stereochemistry—particularly with his concern for the structure and synthesis of sucrose—a subject which later profoundly affected my career (Note 3).

By the time I finished my studies at McGill in the Fall of 1946, the war had ended and I had learned about the existence of postdoctoral studies. The discovery that the antibiotic streptomycin was a carbohydrate seemed to herald the introduction of a whole new field of activity concerned with the role of carbohydrate structures in living organisms. It came to my attention that research on the structure of streptomycin was on-going in the laboratory of Professor Melville L. Wolfrom at Ohio State University, and I asked Professor Purves if he thought I could do postdoctoral studies there (Note 4).

He must have recommended me warmly since by return mail I was asked to present myself in Columbus for an interview for a postdoctoral fellowship sponsored by Bristol Laboratories, Inc., of Syracuse, N.Y. There can be no argument that this move, more than any other, set the pattern for my life (and standard of living) as a research chemist. I not only got a taste for research in the "big league" but started an association with Bristol Laboratories, through its Director of Research, Dr. Amel Menotti, which lasted for over twenty-five years.

It was at Ohio State that I met my wife, Virginia (née McConaghie), who was studying for the Ph.D. degree in high-resolution infrared spectroscopy. We were married in New York City in 1948. In a very real way, "Jeanne" provided all the necessary incentives, repairs and encouragement needed for success in the highly challenging business of chemical research (Note 5).

One day, while in Professor Wolfrom's office, I overheard a heated argument between Wolfrom and Professor W.R. Brode over the evidence for relative configurations of the anomeric centers in carbohydrate structures. Naturally, as a postdoctoral fellow, I was always on the look-out for significant challenges. Because of this unavoidable "eavesdropping", a search for unequivocal evidence for the configurations of anomeric centers relative to those of other chiral centers in the molecule appeared significant. As it turned out, this was the subject of my first research project when I joined the University of Saskatchewan as a lecturer in 1947.

I first became interested in infrared absorption spectroscopy as a tool for organic chemists when we were beaten in the race toward elucidation of the structure of the streptose unit of streptomycin by Folkers and coworkers. Knowing Jeanne and learning about her work helped greatly to reinforce this interest.

I was fortunate indeed to have access to a commercial infrared spectrometer at NRC as early as 1950. At that time, the spectra were generated manually and obtained by plotting the absorptions point by point—a far cry indeed from a modern computerized Fourier-transform infrared recording spectrometer. Nevertheless, the instrument allowed, for example, indentification of L-β-hydroxy-n-caprylhydrazine, a product derived from a glycolipid termed ustilagic acid by comparison of its spectrum to that of the synthetic racemate. It is of interest to note that this appears to be the first application of this since widely-practised procedure for the characterization of an optically active natural product. This 1951 result is of historical interest for another reason; namely, whereas the bioreduction of β-ketobutyric acid, using baker's yeast, produced L-β-hydroxybutyric acid, the similar reduction of the β-keto-n-caproic and β-keto-n-caprylic acid produced the D-β-hydroxyacids. We found this change in stereospecificity very surprising and I kept thinking about it for many years.

B. The intriguing properties of reactions at anomeric centers

While at Ohio State, I had detected the need to develop a better procedure for the preparation of thioglycopyranosides. In Saskatoon, I had a fumehood, ethyl mercaptan, glucose, galactose, zinc chloride, solvents, and few other reagents, and it was possible to undertake a study of the mercaptolyses of the pentaacetates of D-glucopyranose and D-galactopyranose. Thus started a line of research that became a major preoccupation for over ten years.

In 1949, the use of ^{14}C-labelling for tracing reaction routes was still uncommon. We were fortunate to have such methods available in my laboratory for two reasons: one was the association of my employer, the National Research Council of Canada, with the development of atomic energy in Canada and the other was the presence at the Prairie Regional Laboratory of Arthur C. Neish. He was a pioneer in the use of this radioisotope to examine biochemical transformations in plants.

In 1953, Georg Huber and I announced the synthesis of sucrose. This feat, which I will discuss in the following section, won much acclaim. Probably for this reason I was invited to participate in the Fifth Summer Seminar on the chemistry of Natural Products at the University of New Brunswick. D.H.R. Barton was in attendance. Actually, I had already met Derek Barton briefly in Saskatoon where he had visited as a tour speaker for the Chemical Institute of Canada. I remember his visit to my laboratory well because he was very critical. The interest he showed in my work was very satisfying since I was working in the "boondocks" and was devoting most of my effort to studies of reactions at the anomeric center of sugar derivatives. I was just starting my career as a chemist and could ill afford to be wasting my time.

The 1953 U.N.B. Summer Seminar brought me, for the first time, into contact with leaders in organic chemistry. Through these contacts, I learned how truly fascinating and exciting a career in chemistry could be. Also, I developed confidence that I could become involved and compete well enough. Certainly I will always be grateful to Dean Frank Toole and Karel Wiesner for organizing these meetings which became the forerunners of the Gordon conferences in the chemistry of natural products. At a crucial time, these seminars had a major far-reaching impact on the development of organic chemistry—a matter which should be recorded with gratitude.

C. Great luck with proton magnetic resonance spectroscopy

I believe that it was in late 1954 that I first heard a presentation on NMR. The lecture was presented at the National Research Council of Canada in Ottawa by Chris Reid of the University of British Columbia and was concerned with the chemical shifts of hydrogen in fused aromatic structures and how these may be affected by steric interactions. I wondered whether steric effects would cause chemically similar hydrogens that differed in their equatorial or axial orientations to have different chemical shifts. If this was the case, a method could be established to assess the preferred conformations of six-membered ring structures such as the sugar acetates.

Although I had little understanding of the NMR experiment, I approached Drs. W.G. Schneider and H.J. Bernstein of NRC to ask if they would test this idea by examining a few sugar acetates. I soon learned that recording a "high" resolution ^{1}H-NMR spectrum in 1955 was not exactly routine. The spectrometer was a Varian HR-40 which had to be operated with the skill and patience of a brain surgeon. Achieving, finding, and maintaining an homogeneous magnetic field of sufficient size was the main challenge—a situation that was achieved by tightening or loosening large nuts of the yoke of the magnet with a crescent wrench.

Once the basic requirement of an homogeneous field appeared achieved, it often did not last through a run (about 20 minutes) because of a sudden change in line voltage, a passing truck or simply the temperature change caused by someone's opening of the laboratory door. Many of the problems could be minimized by working between 1 and 5 a.m. when the city line voltage was more constant and the street traffic was relatively quiet. Under these conditions, the main worry was: "Would the recorder pen work throughout the experiment?"

In spite of these difficulties and shortcomings, it became evident that the acetylated sugars, because of large chemical shift differences, were very interesting substances for the exploration of the possible use of ^{1}H-NMR in organic chemistry. In fact, it soon became apparent that an equatorially oriented hydrogen generally gave rise to a signal to lower field than that of a chemically similar but axial hydrogen. This was very encouraging. However, the big excitement came when a supervoltage stabilizer was acquired and significant improvements in resolution were made. Under these conditions, it was firmly established, by examination of the spectra for the *cis* - and *trans* -4-*t* -butylcyclohexanols and their acetates, that the spin-spin coupling constants for vicinal hydrogens in *anti* -periplanar orientation were two to three times larger than those for vicinal hydrogens in a *syn*-clinal relationship. This discovery provided the long-sought definitive basis for the assessment of the preferred conformations of the sugar acetates in solution and became a "citation classic". It was clearly apparent that ^{1}H-NMR would have a revolutionary effect on the practice of organic chemistry.

IV. The concentration on methods to synthesize glycopyranosides

A. The synthesis of maltose, sucrose and trehalose and their connection with the Lemieux reagent

My decision to improve the capability for the synthesis of oligosaccharides crystallized at the 1st International Carbohydrate Symposium held in New York in 1951. This was the only occasion when I saw the notorious C.S. Hudson in action when he chaired a session of plenary lectures which included one by "Doc" Wolfrom. In the course of his remarks, he mentioned that a $25, 000 prize had been offered by the Sugar Research Foundation for the first chemical synthesis of sucrose. However, he added that even the simpler structures maltose and trehalose had not been synthesized. Obviously, this was worth thinking about.

The central challenge was to achieve the α-D-glucopyranosyl linkage, a subject which lent itself well to my main preoccupation at the time; namely, the energetics and stereochemistry of reactions at the anomeric centers of sugar derivatives. These studies were all carried out at the Prairie Regional Laboratories of the NRC in Saskatoon, where my appointment had been to investigate the utilization of wheat starch because of the large wheat surplus that then existed.

About a year after I had arrived, the laboratory was visited by the great engineer Dr. C.J. Mackenzie, then President of NRC. I was startled when he took me aside, together with his adviser, Vice-President Dr. E.W.R. Steacie, to state that he appreciated my efforts with starch but really I should feel free to do whatever I wished to do. Wow! This simple vote of trust opened my way to fundamental studies of the physical and chemical properties of carbohydrates and paved the way for my return to academia.

I moved to the University of Ottawa as Chairman of the Department of Chemistry in 1954. There I continued work on periodate-permanganate oxidation, in the capable hands of my first Ph.D. student, Saul Wolfe. As can be imagined from his outstanding career, Saul performed brilliantly. Unfortunately, at the time, I was much too preoccupied with the building of the Department of Chemistry to be sufficiently involved in publication and we were "scooped". Some twenty-five years later, the conclusions reached in his thesis were confirmed in additional studies

B. Syntheses based on additions to acetylated glycals

More and more natural structures containing the α-glycosyl unit became known and the search for improved synthetic methods became more important. Amongst these were the antibiotics streptomycin and kanamycin A whose structure we had established in 1958. Means that would allow the preparation of synthetic or semisynthetic analogs of this or other aminoglycoside-type antibiotics in useful yields then became an important challenge.

Effort in this direction became my main preoccupation on moving from Ottawa to join the Department of Chemistry at the University of Alberta on July 1, 1961. The growth of the Faculty of Pure and Applied Science and the Department of Chemistry in Ottawa had rendered my services as an administrator virtually full time. This left but little time for research or for the writing of publications. Indeed, some of our most important results were communicated only at meetings. Something had to give. The offer of a professorship at my *alma mater*, the University of Alberta, was hard to refuse—there, research would be my main responsibility.

It was painful to leave my friends and colleagues in Ottawa but the call of the West was strong. Large conventional deposits of oil had been discovered in Alberta in addition to the already known enormous deposits of heavy oil and tar sands. As a consequence, Harry E. Gunning appeared to have a *carte blanche* to build a strong Department of Chemistry. I jumped at the opportunity. It is said that I am pathologically Albertan and this could well be the case. Certainly, it was great to return home and to enjoy world-class research facilities. Also, Alberta was an excellent place to raise my family (five daughters and a son) (Note 6).

C. The quantum jump. Halide-ion catalysis. α-Glycopyranosides

The halide-ion catalyzed glycosylation reaction provided a solution to the classical problem of carbohydrate chemistry; namely, the synthesis of 1, 2-*cis*-α-glycopyranosides in high yield. The products of this type of reaction are useful as inhibitors and for the preparation of artificial antigens and immunoadsorbents specific for the human blood group determinants. A measure of its importance is provided by efforts to designate the method simply as anomerization *in situ*. In fact, by the mid-1970's, α-glycopyranosides began to be more readily synthesized than their β-anomers.

D. Phthalimide intermediates for the synthesis of 2-amino-2-deoxy-β-glycopyranosides

By 1973, I had decided to direct my research group toward the synthesis of the ABO and related human blood group determinants. These are complex carbohydrate structures that occur in glycolipids and glycoproteins found at cell surfaces and in biological fluids. The goal was to synthesize the terminal oligosaccharide units—the structural features of interest to antigenicity and to tissue typing. The core structures that lead to these terminal units occur both as linear and branched structures and involve βDGlcNAc units linked either to the 3- or 6- positions of a βDGal unit or to both these positions. Evidently, the synthesis of these complex oligosaccharides would require reliable procedures for the establishment of the βDGlcNAc units. The so-called phtalimide method met this need and has since won widespread application.

V. Stereoelectronic Effects on Conformational Preference and Reactivity

A. The anomeric effect

When Ning-Jo (Paul) Chü came from Taiwan by way of the University of Toronto to the University of Ottawa in 1955 to study toward the Ph.D. degree under my supervision, the conformational properties of acetylated aldoses and aldosyl halides were not known. I suggested that some insight might be gained by determining anomerization equilibria and to examine how these conformed with the tenets of conformational analyses based on nonbonded steric interactions.

In a highly diligent and perceptive manner, Paul studied the anomerization equilibria of the fully acetylated derivatives of seven of the eight aldohexopyranoses and the four aldopentoses. During the course of his studies, it became possible to assess the conformational preferences for the various sugar acetates by ^{1}H-NMR. The results he obtained proved to be compatible with expectations based in conformational analysis but only when a stereoelectronic contribution, which I termed the anomeric effect, was taken into consideration.

Paul Chü's work was never published beyond his Ph.D. Thesis and a communication at San Francisco. The main reason was my involvement with the development of the Department of Chemistry at the University of Ottawa and the design of its first permanent faculty, now known as the Leo Marion Pavillion. We had founded the department in a wooden barrack that had been built during WWI for the Women's Auxiliary Corps of the Canadian Army. Hence, we were very anxious to move out. However, publication was also delayed because we were about to acquire a state-of-the-art 60 MHz ^{1}H-NMR and I wished to confirm, by integration of the signals in the spectra of the equilibrium mixtures, the compositions

Paul had determined by polarimetry. Furthermore, the equilibria for the allose pentaacetates still had to be determined and I wished to re-examine the NMR data for 1-*O*-trideuterioacetyl-β-L-arabinopyranose triacetate at the higher and more stable magnetic field that was to become available.

VI. Further discoveries with and about NMR

A. The numerical signs of coupling constants

Our discovery in 1956 that *anti*-periplanar, vicinal hydrogen atoms are coupled 2–3 times more strongly than when the steric relationship is *syn*-clinical heralded a new era for organic chemistry. The power of the commercial spectrometers rapidly improved and ^{1}H-NMR became an essential tool for competitive research in the general area of natural products as early as 1960. By then we had acquired a 60 MHz spectrometer at the University of Ottawa and I decided that it would be of interest to examine a number of substituted dioxolanes in order to achieve some insight into the conformational preferences. The Karplus relationship for the coupling of vicinal hydrogen atoms was the theoretical base. I was interested in the conformational properties of dioxolanes because of their widespread application in carbohydrate chemistry.

It was found that NMR signals for the ring hydrogens appeared as ABC patterns because their chemical shift differences were not much different from the coupling constants. Under these conditions, the coupling constants can only be obtained by iterative analyses aimed at simulating the observed spectrum. Bob Fraser undertook this chore (computer assistance did not yet exist) but found he could not achieve acceptable simulations.

Bob maintains that I suggested that he try using different numerical signs for geminal and vicinal coupling constants. I do not really remember doing so. What really mattered was that a psychological barrier, imposed by theory, was crossed and the problem was solved. This was the case since it was then generally accepted that all the coupling constants in saturated organic compounds were positive in sign. (Theoretical calculations had predicted that coupling constants would be of the same sign as geminal coupling constants when the latter defined a bond angle less than 125°. Nevertheless, Bob did achieve a thoroughly acceptable simulation of the spectrum both at 60 MHz and 24.3 MHz but only when he used a numerical sign for J_{BC} that was opposite to that for either J_{AB} or J_{AC}.)

We were naturally very excited and promptly submitted this important discovery, as a communication, to the journal that seemed most favored by the theoretical chemists who had recently published on the subject. The paper was rejected on the basis that the work must have been incompetent. A less biased reception was granted by JACS and, indeed, the paper was rapidly accepted and published as a communication to the editor. The finding rapidly gained widespread interest. Indeed, within a year, special symposia on the subject were being organized.

When I moved to Alberta in 1961, John Stevens also came and continued his postdoctoral studies on applications of ^{1}H-NMR to carbohydrate structures. John was supported by the Corn Industries Research Foundation and I wish to record that this organization had a profound influence on the growth of carbohydrate chemistry through both its grant-in-aid of basic research and its annual starch Round Table meetings. This level of altruism by industry no longer seems to exist. This is unfortunate since the essentially "string-free" grants that can be provided by private organizations provide a degree of freedom that cannot be granted by public agencies. I feel very lucky to have been engaged in academic research in the 1950's.

C. Intramolecular hydrogen bonding of oligosaccharides in dimethyl sulfoxide. Deuterium induced chemical shifts

When we had acquired a 400 MHz NMR spectrometer in 1978, Klaus Bock of the Technical University of Denmark came to Edmonton to provide us with the benefit of his experience with a similar instrument. I considered it likely that certain recent publications on the conformational properties of sucrose in aqueous solution were misleading. It was of interest to examine a solution of sucrose in DMSO-d_6 since signals for all eight of the OH hydrogens would be observed as well as those attached to carbon. The resolution was indeed gratifying—the spectrum was a beautiful sight to behold. More important was the discovery that the much improved resolution allowed the observation that a substitution of a hydroxyl hydrogen that is involved in an intramolecular hydrogen-bonded network by deuterium induces a specific chemical shift of the remaining hydroxyl hydrogen.

VII. Synthesis for immunochemical and histochemical studies of human blood group determinants on tissues and in biological fluids

A wide variety of complex carbohydrates occur as building units of molecular structures at the surface of mammalian cells, as components of bacterial cell walls and indeed, it appears, of all biological tissues. In addition, glycoproteins and glycolipids are of widespread occurrence in biological fluids. The roles played by carbohydrate residues in the orchestration of biological processes in living organisms are not yet well delineated, but undoubtedly present rich opportunities for discovery.

The choice of human-blood-group related oligosaccharides as synthetic targets was made because of the great wealth of structural information that then existed. This fortunate state of affairs was due largely to the pioneering efforts of Morgan and Watkins in England and Kabat in the United States. Thus, there existed clearly delineated synthetic goals. However, it was also apparent that success would provide reagents of importance to medical research and products of interest to health care. These latter consideration were, of course, useful for the acquisition of the necessary financial support.

In the beginning, we only had an NMR spectrometer based on electromagnets and operating at frequencies of 100 KHz or less. This situation led to the above-mentioned collaboration with Klaus Bock in 1978. Klaus at that time already had access to a 270 MHz ^{1}H-NMR spectrometer with the highly stable magnetic field generated by a superconducting solenoid. I had become interested in exploring the conformational preferences of oligosaccharides by measurement of relaxation times. Also, it appeared that this new generation of NMR spectrometers would be much more suitable for the measurement of nuclear Overhauser enhancements. Indeed, Klaus Bock contacted me for this very purpose when visiting Laurie Hall during a collaboration. The development led to an exciting, highly productive and pioneering collaboration for the conformational analysis of oligosaccharides. I would never have been able to even try to keep up in the absence of the stimulation provided by Klaus. I was very lucky indeed to be "short" in NMR instrumentation at the beginning of our exploration with complex oligosaccharides.

My engagement with the medical aspects of the ABO blood group determinants became guided by another extremely fruitful and immensely enjoyable collaboration. In the first instance, our work caught the attention of Dr. Jean-Pierre Cartron, of the Centre National de Transfusion Sanguine (the CNTS) in Paris. He wrote to ask whether I could supply a sample of the Type 1 disaccharide (β-DGal(1→3)DGlcNAc), known as lacto-*N*-biose-I, which he desired as a substrate for enzyme studies.

Meanwhile, I had decided that the availability of complex oligosaccharides by chemical synthesis in forms useful for the preparation of artificial antigens and immunoabsorbents should be patented. This would encourage investment in what I deemed an important new technology for the improvement of medical care. This decision brought me into contact with Dr. Roger Perrault, M.D., the director of the Canadian Red Cross blood transfusion center in Toronto, who took an active interest in the potential our research offered to the blood-banking industry. Coincidentally, Dr. Perrault had an active collaboration with the CNTS.

Thus, when I was passing through Paris in the Fall of 1977, I received an invitation to visit the CNTS and to present a lecture. After my lecture, Rafael Oriol, M.D., presented himself with the statement that he wanted to come to my laboratory for three months beginning May, 1978. He came, and through him I entered a whole new exciting world of research—perhaps the most stimulating period of my career. There was so much to learn, and he is an excellent teacher. I had discovered pure "gold". With this background of interdisciplinary collaborations in place, I wish to return to the early 1970's when my engagement with the human blood group antigenic determinants began.

The melding of our chemistry with immunology presented a serious problem since my chemistry students were not particularly enchanted with the idea of learning some immunology. While I did not feel that we should become experts in this immense and highly developed field, I did expect that the development of some immunochemistry at home would be a necessary step toward eventual, significant collaborations in medical science. Clearly we would have to learn the language and be able to acquire at least preliminary experimental data. I made my concerns in this regard known to Claude T. Bishop of the National Research Council, whereupon he strongly recommended a postdoctoral fellow in his laboratory named David R. Bundle. One of the great good fortunes of my career was that Dave accepted my offer of a postdoctoral fellowship in 1973. It is interesting, I believe, that it was Claude Bishop who first whetted my appetite for immunochemistry while playing golf in the rain at a Gordon Conference on carbohydrate chemistry in 1968.

A. Investigation into the Lewis system of human blood group determinants

1. Refined monospecific antibody reagents for tissue typing.

The characterization of the H-type 1 (Le^d) and Lewis c determinants

On immunizing rabbits or goats with the Lewis a (Le^a) artificial antigen, high titers of polyclonal anti-Le^a antibodies were raised. As expected, these antibodies could be isolated from pooled sera by specific absorption using a column of the Le^a immunadsorbent. The antibodies could be desorbed with 1% aqueous ammonia. The eluate was then processsed using standard immunochemical procedures to provide a solution of antibodies that strongly bound Lewis a structures in biological fluids and on tissues.

Our excitement over these accomplishments increased during Rafael Oriol's visit especially when he used the anti-Le^a antibody to detect, for the first time, the Le^a determinant in the epithelial cells of the collecting tubules of the kidney. This was exciting since, previously, sufficiently specific Le^a antibodies were not available. It was the ability to isolate the antibodies from the antisera, using our Le^a immunoadsorbent, that made the difference.

The Lewis system of oligosaccharide determinants occurs in glycoproteins and glycolipids which are characteristic of epithelial cells; that is, those cells which face the outside world (foreign matter). It was gratifying to Rafael to find Le^a determinants in the kidney of an OLe^a patient just at the place where urine is formed—the borderline between epithelial and endothelial cells. To me, it was sheer magic, which reinforced my resolve to contribute to an understanding of the physical nature of such strongly specific molecular recognition phenomena. Along with other blood-group-specific substances, the Lewis structures are important histocompatibility antigens relevant to tissue transplantation. Also, they undergo changes in neoplastic disease.

2. The heretical contribution re ABH secretor status

I found it truly remarkable that certain individuals (nonsecretors) had H determinants on their red cells and vascular endothelia but not in their secretions or their epithelial cells, whereas others (secretors) showed H activity in both regions. Could it be that the secretors had inherited an H gene not inherited by the nonsecretors?

While preparing a lecture, published in 1978, I pointed out that the Type 1 and Type 2 disaccharides present the 2-hydroxyl group of their βDGal units in very different environments. Consequently, in connection with their biosynthesis, I ventured the thought that, "In all likelihood, the enzymes are different." At first, only Rafael Oriol took this possibility seriously. Indeed, he soon established an argument based on genetic data that indicated involvement of two different transferases.

The challenge to achieve biomedical evidence was picked up by Jacques Le Pendu, working in the laboratories of the CNTS in Paris. As expected, the serum of the H-deficient nonsecretor contained no fucosyl transferase activity but other sera considered did. From this, and kinetic studies that followed, it was determined that the enzymes are different and the enzymes responsible for the formation of the H-Type 1 and H-Type 2 determinants arise from different genes. As it turns out, humans and the higher apes show the presence of the H-Type 2 related structures in their red cells and vascular system but not the lower mammals. Baboons are intermediate. Thus, it appears that the *H-Type-2-α*LFuc transferase is an evolutionary marker with the H-Type 1 related enzyme being associated with lower animal species.

B. The new technology

As I have already mentioned, the availability of complete oligosaccharides by way of chemical synthesis has opened the way to new enterprises. Some of the applications are direct such as the use of the immunoadsorbents for the affinity purification of both monoclonal and polyclonal antibodies and, also, of lectins. Also, the immunoadsorbents show promise for the selective removal of antibodies from plasma that may contaminate products such as Factor VIII for the treatment of haemophiliacs and which are obtained by fractionation of blood into its component parts.

There are also indirect applications. Among these are the use of artificial antigens for the immunization of laboratory animals to produce antibodies of known specificities. These are of interest for diagnostic procedures related to blood and tissue transplantations, forensic medicine and the cloning stage of monoclonal antibody production.

Thus the chemical synthesis of complex oligosaccharides made an important contribution to the improvement of health care and my spending most of my time in the period 1973-78 providing leadership in this regard has been justified. It was an extremely demanding but overall a rewarding experience. No one was more amazed than I when a plaque was unveiled in recognition of these contributions at the opening ceremony of the new building erected for Chembiomed Ltd. at the

Edmonton Research and Development Park.

VIII. Molecular Recognition

At long last, in about 1978, the tools required for a meaningful involvement in a study of the recognition of oligosaccharides by proteins were available. Among these were the following:

(1) Our artificial antigens would provide access to hybridoma monoclonal antibodies of known specificity.

(2) Our immunoadsorbents would allow the isolation and purification of monoclonal antibodies from ascites fluids and of lectins from plant seeds. Thus, it would be possible to achieve pure homogeneous solutions of molecules containing the receptor sites for thermodynamic studies.

(3) Synthetic methodologies were well in place for the preparation not only of the natural structure for which antibodies or proteins expressed a strong affinity, but also of modified structures for studies of structure-activity relationships; for example, all ten monodeoxy derivatives of the Lewis b human blood group determinant would become accessible.

(4) It was possible, with the advent of the modern computer-assisted, high frequency NMR spectrometer, to establish the conformational preferences for oligosaccharides and, thereby, gain an appreciation of the topography that an oligosaccharide most likely would present for binding.

(5) A radioimmunoassay using our artificial antigens as the reference structure was well developed and convenient for the measurement of 50% inhibitions, and thereby, the differences in the changes in free energy for different inhibitors.

(6) Techniques were available that could be expected to provide the thermodynamic parameters within useful approximations.

(7) With some luck, it would be possible to prepare the oligosaccharide-protein complexes in crystalline form. Surely, then, collaborations with experts for the sequencing of the proteins and the determination of the crystal structures would be available.

I had considered directing our work toward the binding of oligosaccharides by enzymes. However, it seemed wiser to begin by examining the simpler reversible binding provided by lectins and antibodies. Surely, whatever was found about the complexation of molecules in the ground state would be of assistance in the understanding of complexations that lead to reaction by way of activated states. To be most useful, our work should lead not only to a knowledge of how the complexes are structured but also to an understanding of the forces responsible for their formation so that predictive powers for binding reactions could evolve.

IX. Concluding Remarks

Our explorations with sugars, by my standards, have proved thoroughly satisfying. The various ventures led to numerous virgin territories in the search of knowledge always accompanied by stimulating colleagues and collaborators. We turned a lot of corners.

Much was discovered by accident and often this was more important than what we were looking for. It was an excellent trail to follow which led to most parts of the world. What could be more satisfying than to start a career wondering about the configurational relationship between D-glyceraldehyde and L-alanine and to end up able to profitably wonder about how oligosaccharides are accepted into combining sites presented by large proteins?

One of the reasons for writing this memoir was to show that a career in carbohydrate chemistry can be intellectually very rewarding. I say this because I know that many organic chemists consider that Ray Lemieux's career was in the "backwaters" of organic chemistry. I hope to have corrected this impression and that in the future the subject of carbohydrate chemistry will be granted the high regard that I believe it fully deserves.

The reader hopefully will find my life story of interest since it provides another example of how, given a caring family, a poor boy from the "sticks" can achieve world recognition and the rank of University Professor at his university. I consider this important to recognize since it is the hallmark of a free country. I worry that our freedoms are constantly shrinking.

Like my father who opened a homestead in Lac la Biche, Alberta in 1911, I seemed drawn to pioneering projects. I expect that this is in part because the results of the work you do are better recognized. Mainly, however, I wished to avoid large cities for family reasons and the fact that I always wished to live close to the countryside but not more than ten minutes from work.

Finally, I wish to express my deep feeling of gratitude to all of those responsible for the support of my work and, to a large measure, the freedom to do in my research what I felt best. I well recognize that I stood on the shoulders of many whose names will never appear as co-authors of my publications. My hope is that these have felt a measure of personal success from whatever successes I have had. Over the years, the main sources of my financial support were the National Research Council of Canada, the Natural Sciences and Engineering Research Council of Canada with participation of the Medical Research Council of Canada, and especially the Alberta Heritage Foundation for Medical Research in the very crucial recent times when major funding was required. The concern shown for my work by Bristol Laboratories, Inc. and the now defunct Corn Industries and Sugar Research Foundation was of great assistance. These associations not only provided additional degrees of freedom in my research programs but also many delightful experiences outside of academia.

I have received perhaps more than my proper quota of awards and, for these marks of success, I am deeply grateful. It is noteworthy, I think, that the compliment I best appreciate was from Dean Gendron, University of Ottawa, when he said, "Ray, you are easy to trust." I am not sure what this fully means but I know that trust is what I cherish in people above all else. Indeed, I owe my career to the trust placed in me by my family and employers. It is unfortunate that the element of trust in human affairs seems to have seriously dissipated in recent years. Trust and care are inseparable values and it is difficult to believe that a society can persist in the absence of these most precious human qualities.

X. Notes

Note 1

A number of well-known chemists have offered the unsolicited opinion that I would not have received the financial support necessary for my kind of exploratory research in the United States. Perhaps they were making a valid point since, judging from my experience with NIH study groups, it was certainly much easier, and now even in Canada, to gain support for the study of a clearly delineated objective than for a plan to forage around in ill-understood areas of knowledge in the hope of true discovery. The trend of scientists behaving more like surveyors than explorers has been reinforced by the politicization, in the guise of democratization, of government granting agencies. This development has widened the base for granting support to include not only creative researchers but to some extent even individuals of questionable competence. Thus, the "publish or perish" syndrome has appeared and the tendency to choose projects for their fertility in terms of publications rather that their scientific value is well established. The trend toward conformity and bureaucraticization within well delineated "clubs" which zealously protect their terrain surely presents an important challenge to the healthy growth of scientific knowledge. Such developments are not compatible with the aspirations of highly energetic and creative people who wish to free-range in interdisciplinary activities. It can be expected that fewer of these exceptional people will be attracted to scientific careers. This would be unfortunate indeed, since the need for them in science is greater than ever. The question arises in my mind, "Will it prove desirable to invest in science done primarily to earn a living rather that to experience the joy of discovery and the feeling such engagement brings in terms of *raison d'être*". I doubt it very much. Meaningful curiosity-oriented science is simply just too hard for anything but the completely dedicated effort of exceptionally talented people who are strongly motivated toward the improvement of knowledge for the sake of knowledge. I believe it imperative that a better balance be achieved between the cry for more money to finance basic research and the level of creativity of those crying for greater support.

Note 2

My neighborhood in Edmonton was basically an Irish-French-Ukranian ghetto where the main challenge was to avoid associations that could lead to reform school. My mother died when I was eight years of age and I was largely brought up by my eldest sister, Alice. My father was an itinerant carpenter normally employed throughout the dirty 30's in various towns in the so-called coal branch in the foothills of Alberta. Although I saw very little of him, I was always deeply inspired by his complete dedication to the welfare of his family. I did well in school but until the age of nineteen my main preoccupation was to excel in hockey and play in the National Hockey League. However, after playing the Edmonton Junior Hockey league through Christmas, 1939, I decided that I was not heavy enough to be truly successful in this endeavour. I had registered at the University of Alberta in September of that year and I resolved to find "better living through chemistry". A major consideration in choosing chemistry was that, in the absence of a graduate school, the university employed a number of second-year honors chemistry students as teaching assistants. This added income ($18.00 per month) would be of crucial importance. In fact, I achieved an appointment by leading my class in the freshman chemistry courses. To remain at the University, it was necessary to volunteer for active duty and throughout my undergraduate years, there was a heavy involvement with the officer's training corps. However, because of involvement in war research, I never was called.

Note 3

The most important lesson I learned from Professor Purves was to grant students ample opportunity to read and think for themselves. I feel certain that I could have at least doubled my productivity in terms of publishable work had I pressed harder for the experiments I wanted done. However, I had learned from C.B. (Purves) to grant sufficient rope to either hang or to climb higher and this I tried to emulate. Whether or not I actually succeeded appears debatable. Although I felt that I was always rather easy going and forgiving as a research director, Bert Fraser-Reid loves to announce that I was "both his mentor and tormentor". Nevertheless, I feel sure that it is far more effective for educational purposes to discuss research with the student after he has tried to follow on his own the project you have outlined rather that to guide from the start. I soon learned that a student listens and learns much better after he has made a few mistakes on his own. Whatever I did could not have been so bad, since most of my students developed brilliant careers and we have retained a great level of mutual affection.

Note 4

The fact that my younger brother Gérard died of a streptococcal infection in 1936 at the age of fourteen had a strong influence on my choosing areas for chemical research that bordered on medical science. I had learned from his doctor that his life might have been saved had there been in Edmonton a new anti-infective termed sulfanilamide. At the time, I did not know what this meant, but learning about such pharmaceuticals in my freshman course in introductory organic chemistry certainly strongly influenced my career as a chemist and I constantly drifted toward research on anti-infective medicinals and immunochemistry.

Note 5

Jeanne's and my ability to climb out of a highly budgeted life style came with the founding in late 1962 of R&L Molecular Research Ltd. in partnership with my brother Leo, a local businessman. This came about as the result of an inquiry made by Amel Menotti as to whether or not I could be interested in accepting a major contract to do research focused on the development of semisynthetic antibiotics, starting mainly from penicillin G and kanamycin A. Since I did not consider such a contract to be in the best interest of the university, I replied that I was interested but only if the work could be done in the private sector. Amel then suggested I establish a company to do so. Thus started an exceedingly interesting and exciting ten-year association between R&L and Bristol. The experience gained in company affairs led to the establishment of Raylo Chemical Ltd. in 1966. This company came to own R&L and its main line of business was the sale of custom research and specialty chemicals. In 1973, I decided to sell control to the Canada Development Corporation. In 1976, I sold all my remaining shares to CDC in order to better concentrate on developments in my academic research, for I could foresee commercial interests for which my commercial partner had no interest. After much hesitation, I made the decision to have the technology that I expected would arise from the ability to synthesize complex oligosaccharides, to be exploited by a company owned by the university and which I named Chembiomed Ltd. This decision was reached on the basis of a number of considerations then extant in Alberta. First of all, I felt the university owned intellectual property since it carried all indirect costs and administered my research grants. However, it became obvious that the university, together with a government patent agency, had no real notion of the potential worth of these developments or what to do about them. Furthermore, there existed at the time much talk on campus on how the university's investment in research would lead to local industrial developments with much reference to a pharmaceutical industry. However, it seemed to me that the main promoter had little knowledge about what this would really entail and I decided to use the technology I had in mind to "wet their feet". Harry Gunning was now President of the university and he strongly supported my suggestion that we create a university-owned corporation. Thus, Chembiomed was founded in 1977. As was to be expected, this educational route toward commerical enterprise was fraught with difficulties and highly experimental in nature. Certainly it was not the correct way toward a rapid return on investment. Nevertheless, I felt that, in the longer term, whether it was realized or not, the experience gained in guiding this small enterprise would be of major importance to the development of the university as a better instrument to serve a rapidly growing economy which was in need of industrial diversification. The experiment has been, *to my mind* , a resounding success. The necessary number of mistakes were made. Although the learning curve was very shallow for nearly eight years, the slope increased rapidly in recent years. It now appears that Chembiomed will become a commercial success in addition to having played an important role in the maturation of the university and local government in their attitudes toward high technology. At present, its main product lines are monoclonal antibodies and immunoadsorbents of interest to the blood-banking industry.

Note 6

Of course, my scientific career had to provide support and a feeling of dignity and purpose to the members of my family. Although I consider my children to be my proudest accomplishment, the credit largely belongs to Jeanne. The following list of university degrees earned by our children demonstrates well their breadth of talents and application. There was never a dull moment. Our youngest daughter, Janet, stopped her studies at the B.Sc. level to teach tennis and thereby to have the free time to play on the national ladies' soccer team. She is now back at school in Paris. Laura (b. 1949, now Frost), B.Sc., Ph.D. (Biochemistry); Virginia (b. 1953, now Penny), B. Fine Arts, M.A. (graphic design); Michèle (b. 1954, now Coldiron), B.Sc., M.Sc. (Zoology), M.Ed.; Raymond (b. 1955, married), B.Sc. (Chemistry), B.Eng. (chemical); Andrée (b. 1959, now Lahoda), B. Music., B.Ed.; Janet (b. 1961), B.Sc. (general). Except for Michèle, who resides in Los Altos, California, all have remained in Alberta. Jeanne and I now enjoy thirteen grandchildren. Our main focal point is our summer home in the Canadian Rockies on Lake Edith, Jasper, Alberta. The beauty of the place is simply breathtaking and a constant inspiration.

Acknowledgement

This biographical sketch largely consists of excerpts drawn from the memoirs by R.U.L. entitled "Explorations with Sugars: How Sweet It Was", published by the American Chemical Society, Washington, D.C., in 1990 as one of the series of books entitled "Profiles, Pathways and Dreams, Autobiographies of Eminent Chemists", Jeffrey I. Seeman, series editor.

LOUIS SIMINOVITCH

(May 15, 1920-   )

Killam Laureate, 1981

Lou Siminovitch was born in 1920 in a house in Montreal of parents, Nathan and Goldie, who had immigrated from Eastern Europe in the early 1900's. He lived in Montreal and received all of his formal education there until he received his Ph. D. in 1944.

From 1930 on, the family was one of the very many victims of the depression, which meant that resources at home for educational and cultural pursuits were almost non-existent. Books, records, etc. were in very short supply. But, to some extent, this was compensated for by intense parental respect for education and achievement. Of perhaps equal importance in terms of a future academic career, my parents were extremely family-directed and provided positive reinforcement at every opportunity.

Schooling in Montreal at that time, especially at the secondary school level, was not the very best. For example, science was often being taught by individuals with little or no background in the individual disciplines. Fortunately mathematics could be self-taught; I loved it, and I was good at it. In fact, a very high performance in this area in the general entrance examinations was instrumental in allowing me to meet the unofficial relatively stiff entrance standards for Jewish students to McGill University. Scholarships, bursaries, and early morning paper delivery provided wherewithal for the tuition fees, then, and until I graduated with a B. Sc. in 1941.

The impressions left by one book by Sinclair Lewis (Arrowsmith), together with my generally better performance at maths and science during my high school years, and the model role being played by my brother, David, who preceded me, led me to undertake a science-oriented path at university. My university tutor (a mathematician) dissuaded me from a strongly math oriented career; he felt that positions would be rare in the future for Jews in this area. I therefore chose honours chemistry, where I felt that I could use my math inclinations effectively.

Undergraduate life at McGill was interesting and stimulating in several ways, but two facets are perhaps particularly noteworthy. The honours chemistry course was highly didactic and not especially challenging. And for most of my scientific career, I have not used the information gained therein *directly* in my research. But I believe the in-depth knowledge in chemistry I assimilated during those four years from instructors with considerable experience in their fields has indeed served me well in less definable ways. Fortunately, the curriculum for the B. Sc. extended beyond chemistry and the natural sciences. I was consequently exposed in-depth during this time to the world of English literature, a very new (to me) but long lasting experience. Eventually, during particularly stagnant times in my Ph. D. years, I was able to expand my horizons to include American, and to some extent, world literature, by reading all the books assigned to a couple of English courses. I make this point here because I believe that these latter activities have been extremely helpful to me in developing writing skills, talents which I find are noticeably lacking in many of our present Ph. D. natural science students.

I entered graduate school during World War II, and, again because of my interest in maths, chose to do my Ph. D. in physical chemistry with Dr. Bob McIntosh. During this time I complemented my National Research Council (NRC) Scholarship funds by waiting on tables at resort hotels in the Laurentians. Several important facets of my graduate school experience remain in my memory. Because of the relatively restricted priorities and scope associated with my chemical warfare oriented research problem, I did not feel that I was anywhere near being an accomplished physical chemist when I graduated. However, it was definitely not a wasted three years, for several reasons. I learnt to do research. My supervisor was highly occupied with his own laboratory work and with other matters, and so I learnt to be independent, a characteristic that has served me in good stead throughout my career. I learnt the importance of collegiality, both in respect of my collaborators on my project and my co-graduate students in the same laboratory. And finally, I experienced for the first time the joys, excitement, and self-fulfillment of discovery.

It is difficult to overestimate the importance of chance and opportunity in a scientist's career. Certainly, in my own personal case, there have been many such turning points, and chance has been a recurring theme throughout my career. The first such event occurred during my Ph. D. years. Until then all my choices had been "logical" ones and almost automatic. However, at that point, out of curiosity I attended some lectures in biochemistry by Dr. David Thompson, a superb and highly stimulating teacher. I decided almost immediately that my research interest really lay in life sciences, and that I would orient my career accordingly. It is of interest that, although almost all of my future research work involved studies in biochemistry, genetics, and cell biology, the few lectures in biochemistry that I attended at that time represent the only formal didactic contact I have ever had in the latter areas. Although my experience is not typical, I do believe that we overemphasize structured learning in the training of our research-minded students, particularly in biology. A curious individual will seek out, and select, the information pertinent to his/her interests, and probably assimilate it more effectively.

Having decided to alter my scientific path, how was I to go about achieving such a change? At that time, it was rather difficult to switch into a biology program from a Ph. D. in chemistry, and since I had no idea as to where the expertise resided, I was not in any position to even make a wise choice.

Chance intervened again. Louis Rapkine, an eminent French biochemist, had temporarily forsaken his scientific career early during the war in order to assume the responsibility of science "management" for the Free French forces under General de Gaulle. As such, he was responsible for facilitating or expediting the rescue of scientists from France, locating them in English or North American academic institutions, and overseeing their activities while in these institutions. At that time, the Canadian atomic energy program was situated in Montreal and comprised scientists from several countries, including France. The French contingent reported to Rapkine and thus he visited Montreal often. Rapkine had been brought up in Montreal and was known to me because of a family relationship. It was natural that I discuss my future with him, and he enthusiastically invited me to join him when he should set up his laboratory in Paris after the war; he assured me that he "would teach me all the biochemistry and biology I had to know". He, in fact, discouraged any initiatives on my part to participate in course work.

Rapkine's laboratory was not ready until after 1947, and so, after graduation in 1944, my wife and I were married (one day after we completed working on my thesis together) and I joined the atomic energy project, first in Ottawa at the NRC labs and then at Chalk River. Fortunately, the director of the chemistry laboratories at Chalk River was rather permissive, and I was allowed to choose my own area of research. Because I felt that radioactive isotopes would be of considerable value in biological research in the future, I decided to investigate the development of methodologies designed to prepare carrier-free radioactive isotopes. The availability of the low energy reactor at Chalk River was of course an important resource for these purposes.

The stay at Chalk River was also important because it brought me into contact and associations for the first time with scientists of international calibre from France, England, Italy, and Eastern Europe, and thus kindled my appreciation for excellence. It further whetted my appetite to study in France.

In 1947, I received a Royal Society fellowship, and my wife and I proceeded to Paris where I joined Louis Rapkine who had set up his laboratory at the Institut Pasteur. Although accommodations in Paris left a great deal to be desired, to say the least, the lab itself was relatively well equipped, thanks mainly to the beneficence of the Rockefeller Foundation. I began to learn some biochemistry.

Again the course of events was altered by an unanticipated and unfortunate happening. Louis Rapkine, whom I had come to know and to respect enormously as an extraordinary human being and as a scientist, developed lung cancer (a heavy smoker!) and died in 1948. As he had shown by his willingness to forsake his scientific career during the war, Louis Rapkine was one of those very rare individuals who was prepared to devote considerable time, emotion, and energy to the needs of his friends and colleagues, and fortunately he was blessed with extraordinary wisdom and insight in the conduct of such endeavours. Louis treated my wife and me as part of his immediate family, and even during the relatively short time we knew him intimately, he was able to communicate to us his intense love and social concern for his fellow human beings. It is hard to express in words my overwhelming debt to him and the effects he had on my personal lifestyle and my career. Louis was very well known in Paris and we had met what was probably "la crème" of the scientific community while staying at his home for a few months when we first arrived. Not surprisingly, I was consequently offered several alternative training positions. Because they were highly valued by Rapkine, and were also situated at the Institut Pasteur, I decided to join the laboratories of Jacques Monod and Andre Lwoff with the latter as my primary "patron". I was further influenced in this direction by Sarah Rapkine, Louis' wife, who became my technician for the next five years. In essence, I chose the Lwoff-Monod laboratories blindly since I was totally uninformed in its primary discipline, genetics, and also knew practically nothing about bacteria and viruses, the organisms with which they worked. Andre Lwoff felt the same as Louis Rapkine in respect of my "ignorance" in biology. It was of little concern to him, and his view was "given the appropriate milieu, etc., I would learn on the job".

My stay at the Pasteur turned out to be glorious and inspirational, and seminal in terms of my future career. The study at the molecular level of structure, function, and regulation of viruses and cells was just in its infancy. Because the challenges were well recognized, the field was attractive to scientists of very top calibre from a wide variety of disciplines, including physicists, chemists, biochemists, geneticists, etc. The Lwoff-Monod laboratory happened to be one of two outstanding world centers for this kind of work. The other was situated at Cal Tech in Pasadena under Max Delbruck. These two groups attracted a continuous stream of top scientists such as postdoctorals and persons on sabbaticals and were also in close contact with one another. Further testimony to their excellence came later with the award of Nobel Prizes to Lwoff, Monod, Francois Jacob (another member of the Pasteur group), and Delbruck. As a result, I found myself unknowingly in a superb scientific center, at the forefront of an emerging field which would dominate medical science over the next 4–5 decades. I planned to work in Paris for two years, but remained for six. In the process, I learnt about the power of genetics as a discipline. I learnt, as well, the values of multidisciplinary approaches to biological problems. I learnt the true meaning of excellence, and the levels to which one should aspire. Although each scientist at Pasteur pursued individual objectives, considerable time and priority was given to scientific interchange. I thus again learnt the value of collegiality.

Insofar as my own research was concerned, I was involved in the experiments for which Andre Lwoff later received his Nobel Prize. Two main types of viruses that replicate in bacteria were known at that time. In one case, infection led to lysis and death of all the cells in a culture. In the second, so called lysogeny, the bacteria and the virus were able to co-exist. Lwoff determined the reasons for this coexistence, and together we were able to show that it could be disrupted so that all the infected bacteria now produced virus, and were killed. These studies opened up a completely new focus of studies of the bacterial viruses and provided some of the framework for the eventual development of understanding of regulation in bacteria and viruses and for the later spectacular successes in molecular biology.

As I have indicated above, I grew up as a scientist while at the Pasteur Institut. But concomitantly, my wife and I matured as persons. We left Canada interested in cultural activities but relatively unsophisticated. The difference among my colleagues at the Pasteur was noticeable immediately. We were welcomed and drawn into a host of cultural events, including art, theatre, music, ballet, and opera. Although we were living on the edge of poverty, all of these activities were accessible to low-income individuals. At the Pasteur itself, lunch time conversation among Lwoff's group nearly always centered on politics, or a recent book, art exhibitions, or piece of theatre. Monod was an excellent cellist, Lwoff and Jacob were painters. In fact, new arrivals to the lab were immediately screened by Monod for their ability to participate in trios, quartets, etc. , and I sometimes felt fortunate that I was accepted without having any talents in this direction. Group sorties were organized on a regular basis, and all of this contributed significantly to the collegiality that permeated the laboratory.

As with science, the cultural "education" assimilated in Europe became an integral facet of my future life in Canada. Fortunately, living in Toronto, we have been able to profit relatively readily from the plethora of such activities available.

Leaving Paris was difficult on three counts. First, although I had had complete freedom in my research efforts, even after 6 years I was not overly confident that I could strike out on my own after leaving the nest, especially at the intellectual level to which I had become accustomed. Second, it wasn't easy to find a new scientific home in Canada. I had left as a chemist, and with one exception, I was completely unknown to the biological community. Again, with the same exception, my new field of expertise was almost completely barren in Canada. Positions were available in the USA, but my wife and I decided that we wished to live in Canada. Third, it was not clear that the cultural environment that had became so important to me would be available in a Canadian setting.

Fortunately Angus Graham was working with bacterial viruses at the Connaught Labs in Toronto; we knew each other, and he was familiar with my work. He had no positions at his disposal but managed to convince Dr. Harold Warwick, Executive Director at that time of the National Cancer Institute, to offer me a fellowship. In retrospect I have always been grateful to Angus for his efforts on my behalf, and my gratitude and admiration for Warwick is unbounded, in that he was willing to support me even though my work seemed completely unrelated to cancer.

My years at Connaught were fruitful ones. As at Pasteur, I was able to give my full attention to research, with no competing academic responsibilities. Because of the permissive atmosphere in Graham's lab and the consequent freedom in respect of my research program, and because of adequate resources, I was able to cultivate a completely new scientific interest in mammalian cell biology. The ability to culture cells *in vitro* was just in its infancy, and I felt (justifiably, as it turned out) that the availability of such culture systems would represent essential tools in the future for biochemical-genetic study of mammalian cell function. In fact, as I shall indicate later, the work I initiated at Connaught and the familiarity with cell culture methodology that I acquired provided a basic framework for some of my most important future research endeavors.

Looking back at my three years at Connaught, I am surprised at how much my colleagues and I were able to accomplish in such a short time. At that time, the common technique for growing animal cells in culture was to first attach them to a substrate (glass or plastic). This seemed impractical if one was going to conduct biochemical and genetic experiments; so we devised a methodology to grow such cells in suspension. We then used such suspended cells to develop colony assays for measurement of the numbers of viable cells in a culture and to examine some aspects of the synthesis and the stability of nucleic acids in mammalian cells. With Klaus Rothfels, a cytogeneticist at the University of Toronto, we discovered a novel method for the analysis of the chromosome compostion (karyotyping) of mammalian cells. Previously, cytogenic analysis was performed by "squashing" the metaphase stained chromosomes (applying pressure on the slide by means of the thumb). We found that similarly fine preparations could be obtained by simply allowing the liquid samples to dry on the slide. The relative ease and reproducibility of this methodology was readily apparent and attractive, and it rapidly became the standard technique used for mammalian cell chromosome preparations. Using our methods, we were able for the first time to show that animal cultures could become contaminated in the laboratory by cells from other cultures, a finding which had important impact on laboratory practice for such cultures.

Other than my research, one of the most important outcomes of my Connaught experience was that I came into contact with Dr. Arthur Ham, a professor of histology at the University of Toronto, in his capacity as an advisor to the National Cancer Institute.

During the course of a research career, one becomes associated with individuals who play especially significant roles as mentors, both in respect of science and their personal influence. I had already profited in these ways in my interactions with Lwoff, and particularly Rapkine. Arthur Ham was also special—he was wise, erudite, understanding, giving, and intensely humanistic. I can not adequately express my admiration and gratitude to him for the many hours he spent with me in counsel and support during my stay at the Ontario Cancer Institute (OCI).

In 1956 Dr. Ham was asked to develop a program in cancer research at the (OCI), a new provincial radiotherapy centre being created in Toronto. Dr. Ham understood the role of basic science and the importance of multidisciplinary approaches to the development of understanding of the cancer problem. Thus, even though I knew precious little about cancer, he invited me to join his group, called the Division of Biology, as a staff member, in association with several others in various disciplines such as biochemistry, haematology, genetics, and shortly thereafter, immunology. Thus after 12 years of "training", I received my first academic appointment!!

I spent about 10 years at the OCI. Again, I was fortunate in several respects. The Institute was new and many of the investigators were at the very early stages of their careers. In association with the Division of Physics, we were constituted into a new University of Toronto graduate department, the Department of Medical Biophysics. Thus there was common purpose and enthusiasm in development of the department and in its research objectives. Because of a strong atmosphere of collegiality, collaborations evolved easily between individuals with different expertise, including physicists and biologists. We were a new department, and we were thus able to articulate our own "ethos" of graduate training, in concert. Most important, because of the biology-physics interfaces, we attracted a number of excellent graduate students, many of them with a physics, chemistry, or maths background. It is noteworthy that the prestigious title of University of Toronto Professor has been bestowed on five individuals nurtured in that environment, and that four of these scientists have received International Gairdner Foundation Awards.

In terms of research objectives, biological scientists tend to follow a variety of styles during their careers, from concentration on one specific area of science, to a more eclectic approach moving from one interest to another, as the opportunity arises. In my own career, I have tended to adopt the latter approach. When I arrived at the OCI, I had already passed through several stages—physical chemistry for my Ph. D., nuclear chemistry at Chalk River, enzymology with Rapkine, bacterial virology in the Lwoff-Monod laboratory, and animal cell biochemistry and cytogenetics at Connaught. During my stay at the OCI, I continued in this vein, and was involved in a plethora of areas, including studies with animal viruses, a bacterial virus called lambda bacteriophage, immunology, radiobiology, haemopoiesis, and somatic cell genetics.

In spite of this seemingly broad spectrum of activities, there has always been an underlying thread, that is, the use of genetic concepts and methodologies. Ever since my years in Paris, I realized the power of this discipline in respect of the development of knowledge about all aspects of biology and medicine. Genes are discrete units, and thus lend themselves more readily to quantitative analysis. They are, of course, the units of heredity, and represent attractive objects of study for this reason as well. Either by "spontaneous" events in nature, or by artificial manipulation, one can obtain organisms (mutants) which differ from their normal counterparts by changes in a single gene. Thus one can compare the normal and the abnormal in backgrounds which are completely homogeneous in all other respects. Given the complexity of all biological life, this simplification represents an unparalleled asset to experimental investigation. Finally, as became clearly evident after the discovery of the structure of DNA by Watson and Crick in 1953, the information governing the nature of our cells and how their functions develop and are regulated lies in the compostion of the genetic material. What better way, then, to approach the intricate and challenging mysteries of the biological world?

At the OCI, most of my interest was focussed on three of the subjects I mentioned earlier. Lambda bacteriophage was known to consist of a relatively small number of genes and, because of its limited size, rapid replication cycle, and some other biological properties, it offered an attractive model in which to examine problems of regulation and function of a simple biological system. The key to this analysis was to obtain mutants which were defective in single genes, to then exploit such isolates to compare the normal and abnormal, and to thus delineate their function. Our group, which consisted of colleagues, as well as students and postdoctoral fellows, made significant contributions at two levels, first in the examination of the genes involved in the control of the early stages of the replication cycle, and secondly, in an exhaustive analysis of the genes governing viral morphogenesis, that is, the formation of the viral proteins and their integration into the final rather complex viral structure.

The blood system consists of a fairly large variety of cells, each category having a specific function, and present in the haemopoietic system in different numbers. Information on the development of this system in the late 1950's, and on the function of several of its components, was based mainly on morphological grounds and on descriptive experiments. It was evident, however, that the several categories of cells were derived from a limited number of precursor or stem cells, and that there had to be a constant evolution of cells with distinct function (and morphology) as the cells divided to eventually constitute the blood system. What was desperately needed were quantitative assays for the various categories of cells. James Till, a physicist by background, and Ernest McCulloch, a haematologist, both at the OCI, were successful in 1961 in developing such a technology for the biological assay of stem cells of the haemopoietic system (the spleen colony assay). I joined them shortly thereafter, and the work done by this small group in analysis of the physiology and function of those stem cells, and of their differentiation into many of the other cells of the haemopoietic system, essentially transformed studies in this area. Eventually, it was natural that other biological assays would be developed (by other groups) for most of the other cells in the system. The availability of such assays has had profound influence on studies on haemopoiesis and on understanding of haematological disorders such as leukemia.

My third area of interest at the OCI developed during the latter part of my stay and became the major focus of my research for the remainder of my career. By the late 1960's, genetic concepts and methods had had an enormous impact on investigations into the understanding of regulation and function in micro-organisms such as bacteria and viruses. I had participated in, and of course was familiar with, these advances. Our understanding of such problems in mammalian cells was much less sophisticated, to some extent because little or no genetic analysis had been possible in such systems. It was natural, therefore, that I should consider the possibility of applying the approaches used with micro-organisms to the study of mammalian cells. However, there was one serious barrier to even attempting such studies. Most micro-organisms are haploid, that is , they contain only a single copy of every gene. This means that, if one engineers a change in a single gene (mutation), it is expressed as a visible change in the organism. Much of the success that had been achieved with micro-organisms could therefore be attributed to the fact that, even though mutations were rare, a very wide spectrum of such changes could be isolated in this material. Mammalian cells, however, are diploid, that is, except for the X and Y chromosomes, they contain two copies of each gene. In the majority of cases, for example in cystic fibrosis, a defect in one of the copies of the gene can therefore be compensated for by the presence of the normal counterpart in the second copy. A single defect is therefore not manifest, and disease is only observed when both copies of the gene are altered. These are called recessive mutations.

Since most mutations in animal cells behave recessively, and would therefore require two rare events rather than one, isolation of mutants in mammalian cells would therefore be expected to be difficult. However, having realized that the study of their biochemistry, function, and regulation would be frustrated considerably by the lack of a genetics approach, I decided to "take a chance". I was encouraged to do so because of my extensive familiarity with mammalian cell culture, but mainly because of my earlier experience in karyotyping such cells, where we had found that profound chromosomal rearrangements were common in cells grown in culture. It was possible that these perturbations may have resulted in changes in the numbers of copies of any single gene, and that some might therefore be present in a single copy.

With the aid of colleagues at the OCI and a superb group of postdoctoral fellows, who were also excited by the challenge, we actually did succeed in obtaining mutants, almost simultaneously with another group in the USA. Parenthetically, I believe it is of interest that this work was initiated with funds awarded by a federal granting agency for entirely different projects. Because of its "chancey nature", I doubt whether funding could have been obtained on the project *per se*. Our initial success had considerable impact. Because of its obvious importance and implications, the isolation and characterization of mutants of mammalian cells attracted considerable attention among investigators both in Toronto and elsewhere. In our own setting, more than ten principal investigators located at the OCI, The Hospital for Sick Children, the Medical Science Building and the Banting and Best Department of Medical Research, and a host of students and postdoctoral fellows undertook research programs in the area. Eventually Toronto became of the three most prominent centres in the world for studies in mammalian cell genetics, or as the field is usually called, somatic cell genetics.

In Toronto, we showed that a wide variety of mutants could be isolated in mammalian cells; they affected several diverse functions, and many of them behaved recessively. In theory, this seemed contradictory to established genetic concepts, but we were able to provide very strong evidence that the successes could be explained reasonably. As I had hypothesized originally, it turned out that many genes were indeed present in only one rather than two copies in cells cultured in vitro, and this rendered the isolation of recessive-like mutations in such genes possible.

My work in mammalian genetics was initiated at the OCI but, in the main, was carried out in the Medical Sciences Building on the University of Toronto campus.

The Medical Sciences Building, designed to house the basic science departments, many of the clinician-scientists associated with the teaching hospitals, the teaching classrooms and laboratories, and the medical school administration, was constructed in the early 1960's. The development of this new space was seen by the Dean of the Medical School, Dr. Laurie Chute, and his associates, as an opportunity to correct what were perceived as major lacunae in the academic programs of the medical faculty. Those disciplines included genetics, immunology, basic microbiology, and biophysics; very little research or teaching was being done in these areas on the campus. Since I was considered to have some knowledge and expertise in all of these fields, the Dean invited me to move to the campus to initiate their development, with the promise of several positions and adequate space. I was perfectly satisfied with my situation at the OCI, both sociologically and scientifically. However, I was persuaded to accept the new challenge for three reasons. First, during my later years at the OCI, I had become Head of the Division of Biology, and I felt that it was time to step down. I have always felt that one should not remain in a specific administrative position for too long. A move to the Medical Sciences Building would allow me to continue my scientific activities unabated while providing me with the administrative opportunity to exercise a new leadership challenge. Second, I was aware of the need to develop the disciplines mentioned earlier on the campus. Genetics, for example, was represented on the campus in the Department of Zoology and The Hospital for Sick Children, but in a very limited way, and was not really integral to the medical school. Third, there was considerable enthusiasm for the move by the senior staff and leadership of the OCI, who saw this as an opportunity to develop much closer links between the OCI (one half mile from the campus) and the university.

Although the position at the Medical Sciences Building did not materialize as I had expected in terms of resources, it did provide me with very important new vistas and opportunities. The four disciplines were merged into a Department of Medical Cell Biology. However, because of my own interests as departmental chairman and the nature of the investigators I had recruited, and in order to provide a much more defined focus, we shortly became the Department of Medical Genetics. Under this umbrella, and because I was also named Geneticist-in-Chief at The Hospital for Sick Children (HSC), we were able to develop a highly successful nidus of genetics in the medical school. This, of course, provided me with a great deal of personal satisfaction, in the sense that it accorded genetics a much more appropriate priority in the university. But, in addition, the development of genetics in the medical school was a harbinger of things to come, that is, the overwhelming importance that genetics would assume in all aspects of modern medical research.

In terms of my research, while at the Medical Sciences Building I focussed my entire interest on somatic cell genetics. As I indicated earlier, in collaboration with other colleagues in the Medical Genetics Department and elsewhere in Toronto, and with the aid of several postdoctoral fellows, we were able to develop this area of science into a bona-fide subdiscipline of genetics.

We provided the theoretical framework, in addition to the practical achievements of selection and characterization of mutants of interest, genetically and biochemically. The field was relatively slow to develop and did not become part of genetics textbooks for years, in the main because of the skepticism mentioned earlier in respect of recessive mutations. However, somatic cell genetics has since become an integral part of the genetics armamentarium. These advances were accomplished by developing new methodologies for selection of mutants, by extensive studies on the biochemical and physiological properties of a variety of such isolates, and by detailed analysis of the mechanisms whereby such mutants arise in mammalian cells. I believe it is fair to say that the work of the Toronto group became one of the main foundations for this type of research internationally. As one would have expected, the technology, and the plethora of mutants that have been isolated by ourselves and investigators all over the world, have become an essential framework for a broad spectrum of biological investigation.

I remained in the Medical Science Building (MSB) for 12 years, two of which were against my wishes, but at the specific behest of the Dean. During this period I became increasingly involved in an advisory capacity to science bodies in Canada and the USA. I had already served on a National Institutes of Health Panel while at the OCI, but such commitments became much more extensive. For example, I was active as an advisor to the Medical Research Council of Canada, several bodies in the Ontario Ministry of Health, the National Cancer Institutes of Canada and the USA, the Killam Committee, as well as being an editor of several journals. I mention these activities because, if one is relatively successful, it is incumbent on scientists to "give back" to the system, and, of course, this places increased demands on one's time.

Although I originally was quite concerned about leaving the OCI, in retrospect the move to the Medical Sciences Building turned out to have a very positive influence on my career, in the main because my own horizons were broadened considerably by intimate involvement with the university community. I should mention here that it was during my time at the MSB that the new technology involving manipulation of DNA molecules *in vitro* (recombinant DNA) burst upon the scene.

Recombinant DNA methodology involves the cutting of the large molecules of genetic material into smaller pieces by use of enzymes that act at specific points, then the reassembly (recombination) of such pieces (often derived from different organisms) at will, and finally the reinsertion of the recombined molecules into host cells where they can express their genetic information. This eventually means that one can isolate (clone) specific genetic regions of human DNA, for example. One need not have been too prescient to realize the eventual seminal impact that DNA recombinant methodology would have on future developments in biology and medicine. However, for a number of years I found it difficult to impress this fact on individuals in my own department, in our medical school, and on the Medical Research Council and other scientific bodies in Canada. This was in contrast to the situation in the USA where the technology took hold quickly, both in the universities and in industry. I believe this is a reflection of the conservative nature of our scientific community, to some extent engendered by the conservatism of our granting agencies.

At the end of my tenure as departmental chairman, I decided to move my laboratory operation to the HSC and to assume my position there as Geneticist-in-Chief on a more committed and full time basis. I was motivated to some extent by my desire to again pursue my work in a hospital environment, and, since the major focus of the department at the HSC was in human genetics, this satisfied an interest in strengthening my ties with this discipline. In my own work, it was relatively straightforward to move from the type of genetics we were doing into a more molecular framework, that is, to use recombinant DNA technology to approach the analysis of the genetic structure of some of the mutants with which we had been working in a more detailed fashion.

During this time, almost all my younger colleagues at the hospital switched their interests and programs to the use of the molecular approach. It is of interest that, as head of the department, I came under severe criticism from many of my clinical colleagues in the hospital who felt that our research in the Genetics Department was too basic. Ironically, using recombinant DNA technology, this group has made two of the most significant advances in clinical genetics, involving the identification of the genes for Cystic Fibrosis and Duchenne Muscular Dystrophy. Although a multitude of other examples is available, the events at the HSC highlight the obvious long range importance of basic science.

While at the HSC, I became involved in an advisory role with the Mount Sinai Hospital. While I was playing this role, the Board became convinced that the future of their hospital as an academic institution would be enhanced considerably by the development of a major research thrust. The result was a commitment to build a relatively large research institute and to recruit a research director. I was asked to assume the latter position on my retirement from the HSC in 1985.

I am now the Director of the Mount Sinai Hospital Research Institute, now renamed as the Samuel Lunenfeld Research Institute at Mount Sinai Hospital. In developing this institute and in recruitment, I have once again changed my focus, and have emphasized research in cell, molecular, and developmental biology. This thrust was not new to me since I had had such interests at the OCI, but they resurfaced at Mount Sinai because I feel that they represent some of the major challenges in biology and medicine over the next decade. Also I felt that such research was relevant to a hospital setting. As I had hoped, even in the space of a short four–five years (1990), research at Mount Sinai has flourished under this rubric, and has been attractive to a very large number of trainees at all levels. In fact, because of the importance of genetics in the areas we chose to develop at Mount Sinai, many of the new staff naturally became integral members of the Department of Medical Genetics. As a result, the Department itself has increased even more in stature.

My decision to move to Mount Sinai was also motivated by the social dynamics of modern medical research. Increasingly, as we had observed at the HSC, the hiatus is narrowing between advances in basic research and their application. This is particularly true in biology and medicine. In my view, these changes are best exploited in a hospital where one can bring basic scientists and clinicians into close contiguity.

I hope to culminate my career at Mount Sinai!!

In conclusion I wish to make some general remarks about my scientific career. As I hope I have been able to communicate, it has been a very rich experience. In several respects I have been extremely fortunate. Fortunate in being able to express myself scientifically in a variety of directions and settings. Fortunate in being associated with leaders who recognized the benefits of permissive atmospheres in which to do research. Fortunate in often finding myself in forefront fields of biological science. Fortunate in having serendipitously chosen genetics as a focal interest. Fortunate in having been associated with colleagues and a host of students and postdoctorals of top quality. And finally, fortunate in having been able to do this *pari passu* with opportunities to provide leadership and advice in various capacities in Toronto, Canada, and internationally. My involvement over the years with a host of agencies, committees, and journals has taken its toll in time and effort, but this has been more than compensated for by the satisfactions of accomplishment and by the host of individuals of varied backgrounds with whom I consequently became associated. Perhaps the greatest satisfaction comes from observing the success of many of the young people one has trained or recruited into staff positions.

Science is an extremely challenging profession, and in a country such as Canada, where its value is not well recognized, especially so. However, particularly in biology, the intellectual opportunites have never been more promising. Would that I could begin a second career!!

WILLIAM THOMAS TUTTE

(May 14, 1917–)

Killam Laureate, 1982

I have been asked to write an account of my scientific career for the Science Council of Canada and the Killam Program of the Canada Council. It is to be an inspiration or a warning to young people considering careers in Science. I now attempt a response to this challenge. In the main it will be similar to an account already made for the Royal Society of London. Greetings, young scientists.

I record first that I was born on May 14, 1917, at a place called Fitzroy House in Newmarket, England. I have understood that my father was employed as gardener and my mother as cook in that house. But I note that on my birth certificate my father's occupation is described as "Caretaker of mansion and gardener (domestic)".

I have no early memories of Fitzroy House, for my parents soon left it. I associate my early childhood with a house called Moorend, high on the moor over the Yorkshire village of Aislaby, three miles inland from Whitby. There my parents were, once again, caretakers. The owner used the house for relaxation, for entertainment and for moorland sport, but he was not permanently in residence. Curiously I can revive no definite memory of him or his guests, save for one lady who presented me with some books of Beatrix Potter.

It was at Aislaby that I first went to elementary school. It was only for a few months, until my parents left Moorend and returned to the neighbourhood of Newmarket. I remember lessons in reading there. I took in them only a detached interest, for my parents had already taught me to read. In other lessons there was much making of change with play-money. Would this be an early exercise in Mathematics?

My parents bought a cottage in Cheveley, a village three miles from Newmarket. Their cottage was one half of a flint building, accessible only by a foot-path through the grounds of a third cottage, a thatched building of lath and plaster. My father carried on his profession of gardening. I particularly remember the years when he tended the garden of the Rutland Arms Hotel in Newmarket. Alas, that garden is now gone, and a supermarket occupies its site.

I attended the elementary school in Cheveley, another flint building. Flint is plentiful in that district. I cannot now give any coherent description of the course of my education there. At some time during my early schooling I was introduced to the prime numbers, and I was much fascinated by them. I learned how to find the highest common factor of two integers, using what I was later to call Euclid's Algorithm.

Later I was introduced to ruler and compass constructions. I learned how to bisect segments and angles, how to divide an angle into four equal parts, and how to partition a segment into any desired number of equal parts. But I was much perplexed because my little text-book entirely ignored the related problem of dissecting an angle into three equal parts. I wonder now what it ought to have said about that intractable problem?

On the walls of one classroom there hung depictions of animals that were strange to me. These were never the subject of a lesson, but somehow I came to realize that they were animals that had lived in past ages.

There was a small school library wherein I had access to the volumes of a childrens' encyclopaedia. I was fascinated by the astronomical articles and, to an only slightly lesser extent, by the essays on other sciences. Thus I set out on the upward path, or as they might say in the Other Culture, the slippery slope, that leads to a scientific career and the portals of the Science Council of Canada.

Geological information inspired me to explore the local chalk pits in search of fossils. Cheveley is a great place for fossils and Whitby, which we still visited from time to time, is famous for them.

From my scientific reading I got the same kind of thrill, though at higher intensity, that I had got from the first part of the Book of Genesis when I was first commanded to study that work. I still think there is something of scientific interest in it. Is there not a foreshadowing of evolutionary theory in the story of the successive creation of different kinds of living things? First plants, then the creatures of air and water, then the land animals and then people. Is there not another such foreshadowing in the story of the creation of Eve? There God, wishing to create what the writer regards as another species, makes her, not out of nothing nor yet from the dust of the ground, but from the tissues of a species already existing. It would be easy to welcome the writers of that first part of Genesis as scientific colleagues. All that prevents us is the refusal of too many of our spiritual pastors and masters to accord to those ancient philosophers the scientist's qualified right to be wrong.

I was less happy with some other parts of the Bible. When I read the Book of Joshua, I sympathized with the Canaanites, defenders of their country. With the Book of Job I was more concerned about Job's sons, uncaringly slaughtered to test another persons's faith, than about Job himself. In those matters I think I went against the intentions of my instructors. I mention them because I had deviations from orthodoxy over scientific questions too.

I would not describe myself as a rebel. On the whole I was willing to swallow such mental food as was presented to me. But some things just would not go down. For example, I was told that the sky did not really rotate; it was the Earth that rotated under it. And I did not doubt that information. But I could not swallow the arguments that were said to prove it. Alas, I did not get so far as the proposition that all rotation is relative; I made no independent discovery of the principles of Relativity. But it did seem to me that something was missing. I did not discuss my misgivings, whether theological or mathematical, with my teachers. I think I knew by experience that I would get more ridicule than satisfaction. That left me with no recourse but to think for myself, which may have been a Good Thing. I had found the ability to doubt an alleged proof, and that ability is essential in mathematical research.

The years rolled by and I was ten. I was thought to have done well in my school work, having clung pretty consistently to second place in the mark list. My parents were therefore told that I ought to move on to a secondary school. It would be necessary to pass a scholarship examination, and this I did. It seemed that my new school would have to be not in nearby Newmarket but in distant Cambridge. For Cheveley was in Cambridgeshire and Newmarket in the county of Suffolk. Every school-day morning I would have to bicycle to Newmarket station, travel by train the fourteen miles to Cambridge, and then walk a mile to school. All this was judged to be too strenuous for me and I remained at the elementary school for another year. Then the procedure was repeated, and this time I was enrolled as a pupil at the Cambridge and County High School for Boys. That was in 1928.

At that school I was introduced to some new subjects, notably French and Latin. Now, I can read French but not easily converse in it, and I can sometimes get a sketchy idea of the meaning of a Latin inscription. Later I was given lessons in German. Now, I can read scientific German, given a strong enough motivation, but Heaven forbid that I should ever have to write any. I remember with pleasure my courses in English History. But I was chiefly interested in the scientific subjects, including Mathematics. In Chemistry, Physics, and Mathematics, I was usually near the top of the class.

One stimulus that now seems to me supremely important came from outside the regular curriculum. In the school library I came upon a copy of Rouse Ball's "Mathematical Recreations and Essays". Therein I found much information about Graph Theory and more general Combinatorics. I read the basic theory of the Four Colour Problem and a discussion, without proofs, of Petersen's work on cubic graphs. That was my first encounter with the subject in which I was later to specialize. I was delighted, many years later, when H.S.M. Coxeter, as editor and author-in-succession, asked my help in bringing Rouse Ball's book up to date.

In 1933, at the age of sixteen, I had to decide whether to leave school or to stay on for another two years preparing for University. I stayed on, my Scholarship continuing.

I passed more examinations, and in 1935 I became an undergraduate at Trinity College, Cambridge. I was adequately supported financially by a State Scholarship, a College Scholarship, and a grant from the County. I was to study the Natural Sciences, proposing to specialize in Chemistry. I suppose I applied myself fairly well to this work, for I passed the final examination with First Class honours. Nevertheless, as time went on, I yielded more and more to the seductions of Mathematics.

One of my mathematical friends at Trinity came upon an intriguing geometrical problem: is it possible to dissect a square into smaller squares, all of different sizes? That problem was at the time unsolved. But there was an example in the literature of what I later came to call a "perfect rectangle", a rectangle dissected into unequal squares. In this case there were nine of these component squares. I became a member of an association of four undergraduates who spent much time constructing perfect rectangles, representing them by graphs, applying Graph Theory to their study, and eventually finding constructions for "perfect squares". There was R.L. Brooks, later to make his career in the Civil Service. There was C.A.B. Smith, later of the Galton Laboratory where he achieved fame in Statistical Genetics. The fourth member of the association, and the one who discovered our problem for us, was A.H. Stone, later a topologist and a Professor of Mathematics at Rochester, N.Y. Our work on perfect rectangles and squares was published in 1940, in the Duke Mathematical Journal.

We did this research for fun. I think we were inhibited by a mathematical fashion of the time, according to which Graph Theory was a triviality notable only for a few amusing puzzles. Only belatedly did we come to believe, really believe, that we were doing serious mathematics. Had we come to this belief earlier, we might have published earlier and been the first to publish a perfect square. As things were, that honour went to R.P. Sprague of Berlin, who published an example in *Mathematische Zeitschrift* in 1939. In this extra-curricular work I got a thorough grounding in Graph Theory and laid the foundation for much of my future research.

I took my B.A. in 1938 and began research in Physical Chemistry, first in infrared spectroscopy with G.B.B.M. Sutherland and then in the ultra-violet kind with W.C. Price. There is a paper by Price and Tutte in the Proceedings of the Royal Society for 1940.

It had become clear, however, that I was not going to shine as an experimental scientist. I had a keen interest in the Natural Sciences, and this had got me easily through many examinations, but it had not given me experimental skill. On the other hand, I had established myself unofficially as a mathematician. My tutor Patrick Duff was able to arrange my official transfer from one subject to the other. I left Chemistry with an M.Sc. degree and with the intention of devoting myself henceforth to research in Mathematics. But there was a war on.

There came to Patrick Duff an enquiry: did he have any puzzle solvers suitable for training in Cryptoanalysis? He thought I met this description. So early in 1941 I found myself taking a course in Cryptography in London. After that I was received into the Research Section at Bletchley Park to work with Captain, later Major, G.W. Morgan. Officially I was employed by the foreign Office, which bestowed upon me the title of Temporary Assistant Clerk.

Wartime Bletchley Park, once darkly secret, is now famous and its achievements are well known. I find some of them described in a recent biography of Alan

Turing. There is even a mention of my first cryptographic success, which helped in the breaking of the "Fish" machine.

This essay is about my scientific career and not about the course of the Second World War. So I mention only one minor consequence of this cipher-breaking: it gained me a Fellowship at Trinity College. The election was, I am told, something of an oddity. One or two of the electors were associated with Bletchley Park. The others took their word that I was a suitable candidate.

Some of the additive ciphers of those days could be regarded as exercises in Linear Algebra. So, when I returned to Cambridge in 1945, I had a close acquaintance with that subject, or at least with some of its by-ways. I had also learned something of the uses of Statistics.

Back at Trinity, I worked for my Ph. D degree. I published four papers in 1946–47 that were deemed important by later writers. My thesis, which did not incorporate them, attempted to reduce Graph Theory to Linear Algebra. It showed that many graph-theoretical results could be generalized as algebraic theorems about structures that I called "chain-groups". Essentially I was discussing a theory of matrices in which the standard elementary operations could be appled to rows but not to columns. Much of this thesis, rewritten, I was able to publish later as an extension of Hassler Whitney's theory of "matroids".

I try sometimes to uncover the reasons for my success in these early graph-theoretical studies. It was due, I then decide, to the fact that I was working, full-time and with enthusiasm, in a largely unexplored field. I think of a gold-field from which travellers have already removed a few nuggets, but where many remain for the first persons willing to stay and dig well below the surface. Hassler Whitney had done that a few years before but then, perhaps fortunately for me, had left the field for other provinces of Mathematics. I think that other potential developers were repelled by the ill-repute of Graph Theory, the "slums of Topology", and had been urged by their supervisors to turn to mathematics of a more savoury kind. I too was urged by people older and wiser than myself to work on something respectable like differential equations, or at worst the classification of the 3-dimensional manifolds. In my case, the pressure was no doubt weaker because I had already demonstrated that I could get results that were interesting in spite of being graph-theoretical. Moreover, as a Fellow of the College, I had more prestige than the usual beginning graduate student. Here let me assert, my dear readers, that Mathematics is One. König found Graph Theory in Set Theory and I have found differential equations in Graph Theory.

When I had written my thesis and had defended it before the examiners, it was time to look for a job. I proposed to teach Mathematics at a University, but there seemed to be few openings. I remembered that Professor Coxeter of the University of Toronto had shown interest in my graph-theoretical work and I applied, through him, for a post there. I was accepted and my fourteen-year association with the University of Toronto began. I was called in the first years a Lecturer, then an

Assistant Professor, and in the last two years an Associate Professor.

Having got my hero into an ordinary academic routine, I am now tempted to cut short this story of scientific adventure. But I must record that in 1962 I was persuaded to become a Professor at the new University of Waterloo, where I was to concentrate on the development and the teaching of Graph Theory. For that subject had by now become respectable. Moreover, I was told, it was useful in Computer Science, in Optimization, and in Communication Theory. Even matroid theory, at least the part I had worked on, was valued by electrical engineers. I had acquired the reputation of a pioneer in the newly popular discipline and honours came to me accordingly. Fellow of the Royal Society of Canada in 1958, Killam Laureate in 1982, and Fellow of the Royal Society of London in 1987. And, in 1988, I was asked to write this essay.

I look now at the list of topics I have been asked to touch upon. Some I have dealt with as well as I can. One heading is "Why Science?" and that puzzles me a little. Is it supposed that between the ages of fourteen and sixteen I must have given deep thought to the problem of whether I wanted to be a scientist or a poet, a preacher or a linguist, a soldier or a businessman? Not so. I had been made for Science and I had no choice in the matter. I was aware by then of certain strictures of fundamentalist religion and snobberies of the Other Culture, but I cannot remember ever attaching any significance to them.

I wonder sometimes whether many or any budding scientists were driven away by those cries of "Yah! Barbarian!" from beyond the cultural divide? I do have the impression that those who chose Science in those days were going against an intellectual fashion, just as those scientists who chose Graph Theory were going against another. Let us have a moral. Beware, my children, of intellectual fashions.

I am asked for my most exciting and most frustrating research experiences. Well, I am always excited when I discover a theorem, but all those excitements have been on much the same level. I had a typical high when I found the necessary and sufficient condition for a graph to have a 1-factor, the result that textbooks now call "Tutte's Theorem". The frustrating experiences involve various approaches to the Four Colour Problem or its generalization, the Hadwiger Conjecture. For example, there is an inviting approach to the Four Colour Problem that starts by applying mod 3 algebra to the formulae known as Heawood congruences. It lures one on from false promise to false promise, brings one to what seems to be a pinnacle of achievement and then wrecks one's hopes upon some algebraic subtlety. It would, I feel, be easy to get obsessed in this Never Never Land and lose one's mathematical sanity. I came to think that was happening to me and I abandoned the investigation forthwith. You young people should be able to make such an abandonment and to know when it is necessary. That is the most important lesson that I can cull from my experience to pass on to you.

Family details? I met Dorothea Mitchell in 1948, very soon after my arrival in

Canada, and we were married in 1949. There you have one reason why I stayed in this country. My father died in 1944, my mother in 1956, and my only brother in 1977. My brother's children still live in Newmarket, which Dorothea and I often visit. There is now a third and fourth generation of that branch of the family.

Hobbies, etc.? Nothing out of the ordinary there. Some gardening, some travel. Dabbling in games such as chess, go, bridge, and scrabble, but not to the extent of becoming really proficient in any of them. I like reading History too. For many years, beginning in 1938, I took an active interest in the Youth Hostels movement, often going hostelling in England and, from 1948 on, in Canada. I should have mentioned above that it was as a fellow hosteller that I first met Dorothea. It is also partly true that teaching has been my work and research my hobby.

At the last I am asked to comment on the future, and in particular, the future of Canada and your involvement in it. That would have been routine forty years ago. The old man who was pontificating would have advised me as to what branch of Science or Mathematics was likely to be most lucrative. Differential equations, perhaps. Rising for a while above that consideration, he might have spoken of the need to expand Canadian agriculture further into the Northland by breeding new varieties of wheat, or the need for a better travel service to the Far North. I could still speak of those things, but I feel there has been a change of emphasis. In the future Canada's problems are likely to be mere local aspects of global ones.

I read of an increasing population that is likely to outrun its food supply. I read of a greenhouse effect that is likely to bring drought and desert to the world's most fertile fields, those not already encroached upon by supermarkets and the like. I read of a spreading pollution that afflicts much of the vegetation beyond those fields.

I read also words of comfort. Even *per capita*, the production of food is still increasing. Pollution can be combatted, since even the River Thames has been cleansed. Nevertheless, Earth's population is expected to double in the next forty years. Hence, today's young people must contrive to double food production in those forty years. I reflect upon what agriculturalists have won for us in unremitting toil through hundreds of generations, and then reflect that you are expected to do as much again in your own lifetimes. Moreover, and this is the really tricky part, you had better do this without further damage to the environment, and even while repairing some of the damage that has been done already.

The problems of the future are daunting indeed. But no doubt you will cope with them, in cooperation with your colleagues of other disciplines. Clearly, much applied Science will be needed. Perhaps I am prejudiced, but I suppose basic Science will be needed just as urgently. Even those who dissect squares or study other planets will have their contribution to make. We can already turn to our sister planet for an indication of what may happen if we wrench our biosphere so far from its former equilibrium as to drop it into another.

Artistically that would have been a good place to stop; so count this paragraph as a footnote. Sometimes a teacher says that every mathematical student ought to be acquainted with Subject X, and therefore a course on Subject X needs to be crammed into his already bulging curriculum. Or someone may complain that scientists are ignorant of "values" and should therefore be required to take extra lessons on Remedial Morality. Both assume that Knowledge and Wisdom can be pumped into a student's brain by relays of lecturers in whatever manner and to whatever extent some planning authority may decree. I then reflect that I am reputed to be a mathematician, and even a leading one, although I never took the regular course in Mathematics. I only took the diluted version deemed adequate for Natural Scientists. I learned mathematical research by doing it for fun with the other members of the "Gang of Four". There is a contradiction here, but I leave its resolution to future educationists. Meaning, of course, yourselves.

BRENDA MILNER

(July 15, 1918 –   )

Killam Laureate, 1983

Dans les abîmes de la mémoire[1]

Dans l'univers de la science, il subsiste encore quelques « boîtes noires », des énigmes qui, lorsqu'on croit en avoir enfin compris les mécanismes, témoignent du gouffre profond de notre ignorance. La mémoire est l'un de ces phénomènes mystérieux dont la mécanique complexe nous échappe. Cette faculté du cerveau, si précieuse pour la pensée humaine, a captivé l'esprit scientifique de Brenda Milner, neuropsychologue.

Fondatrice et actuelle directrice du Département de psychologie de l'Institut neurologique de Montréal, Brenda Milner a transformé la neuropsychologie en une science expérimentale. Ses études sur les patients de l'Institut, opérés pour des problèmes graves d'épilepsie, ont aidé à fournir des diagnostics plus précis, mais ont aussi grandement contribué à accroître les connaissances sur le fonctionnement du cerveau.

Par ses travaux, Brenda Milner a réussi à cerner le rôle de l'hippocampe dans le processus de mémorisation et à démontrer l'importance du lobe temporal droit dans les perceptions visuelles et auditives. Ces dernières années, la neuropsychologue a élucidé les fonctions d'organisation temporelle des lobes frontaux, l'une des régions du cerveau les plus difficiles à mettre en relation avec le comportement humain. À 72 ans, et après 50 ans de carrière, Brenda Milner est parmi les chercheurs canadiens les plus susceptibles de recevoir le prix Nobel.

Assise, toute menue, sur sa petite chaise de bois, Brenda Milner parle de ses recherches comme s'il s'agissait d'un roman policier. Elle s'anime et son discours, sérieux, est parfois entrecoupé de petits rires de jeune fille. Mais sa grande curiosité pour l'être humain n'a plus rien d'enfantin; c'est un esprit cartésien qui porte son regard sur la vie. « Ce qu'il y a d'extraordinaire chez elle, c'est cette curiosité si particulière », raconte son collègue Michael Petrides, professeur agrégé au Département de psychologie de l'Université McGill. « Son intérêt suprême dans la vie est de recueillir des données de ses malades et de les examiner, passionnément, quelquefois jusqu'aux petites heures du matin. Et quand surgissent des données vraiment intéressantes, elle devient tout excitée, comme un enfant avec un nouveau jouet. »

En route vers la recherche

La vie de Brenda Milner, née Langford, n'a rien d'un labyrinthe. Elle s'est tracé une voie, bien droite, vers un but: l'excellence. Déterminée, elle n'a pas eu peur du risque. À 18 ans, à l'encontre des désirs de sa mère qui la destinait à l'étude des langues, elle choisit d'étudier les sciences à l'université la plus réputée à cette époque: l'Université de Cambridge. Malgré la forte concurrence, elle est la seule à n'avoir pas formulé de demande d'admission ailleurs, pas même à Oxford.

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[1] Cette biographie est adaptée du texte tiré du livre *Une Passion: la SCIENCE: Portraits de pionniers québécois* par Claire Chabot (Éditions MultiMondes, 1990), avec permission.

Trois ans plus tard, c'est comme première de classe qu'elle obtient le B.A. anglais en psychologie expérimentale. « Mais c'est parce que j'avais un but, explique Mme Milner. Je voulais faire de la recherche et il fallait pour cela que je reçoive une bourse. J'y ai mis toute mon énergie. » Elle semble vouloir convaincre que sa réussite ne dépend pas de son intelligence supérieure. Son raisonnement de spécialiste ne la quitte donc jamais: elle connaît bien le rôle de la volonté et du travail dans le processus d'apprentissage.

Dotée d'une bourse de recherche, la jeune psychologue anglaise entreprend des travaux sur la perception visuelle. En septembre 1939, la guerre éclate en Europe. Nommée officière dans le service civil, elle oriente ses recherches vers des besoins plus immédiats et utiles à son pays, la Grande-Bretagne. Elle étudie les sensations et les perceptions, parfois illusoires, que ressentent des pilotes de chasseurs à partir des indications de leurs instruments de vol; ses observations lui serviront à créer des tests qui permettront aux pilotes de s'orienter soit vers les chasseurs, soit vers les bombardiers.

En 1941, Brenda Langford obtient un poste dans un laboratoire de recherches expérimentales sur le radar. D'abord situé sur la côte sud de l'Angleterre, le laboratoire est rapidement transféré en pleine campagne, dans un lieu secret, de peur qu'on vienne s'emparer des grands « cerveaux » spécialistes en physique et en mathématiques qui s'y trouvent. C'est là qu'elle rencontre son mari, Peter Milner, un ingénieur devenu psychologue, qui invente un appareil pour enseigner à faire fonctionner des radars. Avec lui, Brenda Milner élabore des méthodes d'apprentissage pour détecter les avions et les missiles.

Puis, un jour, Peter Milner est appelé à aller travailler un an au Canada, au laboratoire secret d'Énergie Atomique à l'Université de Montréal, avec une équipe de physiciens dont fait partie John Cockcroft. Ce départ précipité force Brenda et Peter Milner, devenus de grands amis, à se marier. « Je n'avais jamais pensé me marier, raconte Mme Milner, mais en ce temps-là, on n'accompagnait pas un homme en amie. »

À Montréal, cependant, une surprise attend Brenda Milner. Son diplôme de B.A., même avec une mention honorable, n'impressionne guère; on exige une maîtrise et un doctorat. « À Oxford et à Cambridge, explique la psychologue, la maîtrise n'a pas la même signification. Après un certain temps, une probation de respectabilité, on pouvait acheter le diplôme. Le M.A. coûtait cinq livres sterling et je n'avais pas jugé bon de débourser une telle somme, à l'époque. » Finalement, l'Université de Montréal lui offre un poste de professeure à l'Institut de psychologie: son français, qu'elle avait pratiqué en lisant la littérature française et des livres de mathématiques, lui ouvre les portes. Elle donne un cours sur la mémoire d'après le système de son professeur de Cambridge, F.C. Bartlett, et un cours sur le comportement animal.

Ouvrir la boite noire du cerveau

Partageant son temps entre l'enseignement et la recherche dans son laboratoire de l'Université de Montréal où courent des rats dans des labyrinthes, Brenda Milner doit se résoudre à faire son doctorat. Elle sert de personne-ressource dans des séminaires dirigés par Robert Macleod, du Département de psychologie de l'Université McGill. Celui-ci, à cette époque, fait venir le psychologue canadien Donald 0. Hebb, qui va attirer rapidement autour de lui un groupe de brillants chercheurs. Brenda Milner « décide » qu'il sera son directeur de thèse.

Donald 0. Hebb revenait à l'Université McGill d'une tournée des grandes universités et des principaux centres de recherche en psychologie: Chicago, Harvard, Yerkes Laboratories of Primate Biology. À son arrivée, en 1947, il apporte le manuscrit de son livre qui le rendra célèbre, *The Organisation of Behavior*, l'une des premières tentatives d'explication des phénomènes psychologiques par le fonctionnement du cerveau. Utilisant les meilleures notions d'anatomie et de physiologie de l'époque, il a créé des modèles pour relier les comportements perceptifs, émotifs et cognitifs à l'organisation du cerveau.

L'influence de Hebb sur Brenda Milner n'est pas directe: le psychologue tient sa renommée de ses théories, beaucoup plus que de son expérimentation. « C'était un grand théoricien qui, aujourd'hui encore, a une influence énorme sur la psychologie et la neurologie, affirme Brenda Milner. Les physiologistes de son époque pensaient que ses modèles étaient de la pure spéculation. Ils nous déconseillaient de faire de la psychologie physiologique et ils avaient tort. Aujourd'hui, il est pris au sérieux. Quand on ouvre n'importe quel livre de science neurologique, on parle des synapses de Hebb et de ses modèles. »

« À cette époque, dit pour sa part Michael Petrides, la majorité des psychologues regardaient le cerveau comme une boite noire: oublions ce qu'il y a à l'intérieur, mais essayons de décrire les comportements! Par contre, les neurologues, qui connaissent le cerveau, se sont toujours intéressés au comportement. » C'est en 1861 que Broca a décrit pour la première fois des troubles du langage en relation avec les dommages causés au cerveau. Il a défini une zone du langage, l'aire de Broca, située dans le lobe frontal gauche. Par la suite, les neurologues, souvent fascinés, ont continué à décrire les troubles de leurs malades atteints de tumeurs ou de traumatismes crâniens.

En 1950, Donald Hebb propose à Brenda Milner d'étudier les patients de l'Institut neurologique de Montréal. Hebb, qui avait passé deux années à l'Institut, avait fait promettre au Dr Penfield de recevoir un de ses étudiants, voyant là des possibilités intéressantes de recherche. « Brenda Milner, raconte M. Petrides, fut l'une des premières psychologues à entrer dans un hôpital neurologique pour étudier systématiquement des malades ayant des troubles circonscrits au lobe temporal. Elle a analysé la nature de leurs problèmes en utilisant la méthodologie de la psychologie expérimentale. C'est vraiment une pionnière en neuropsychologie. »

« Pendant longtemps, mais beaucoup moins aujourd'hui, explique Brenda Milner, les médecins ont eu tendance à traiter la psychologie comme un sens commun que tout le monde peut comprendre. Mais l'essence de la psychologie est de mesurer les comportements pour les distinguer et, pour cela, il faut élaborer des tests et analyser de façon méticuleuse les données recueillies. La majorité des médecins, comme tout le monde, ont une vue globale du comportement des individus. Parce qu'une patiente peut paraître très bien élevée, répondre avec courtoisie et être bien habillée, le médecin va souvent surestimer son niveau intellectuel. Tandis qu'une jeune femme plus introvertie, moins attirante et venant d'une famille pauvre sera peut-être sous-évaluée, malgré son succès dans les tests. Dans ces conditions, les résultats obtenus par le médecin et le psychologue pourront se contredire. J'étais tellement furieuse quand je voyais dans certains dossiers: arriéré mental! »

H. M.: un amnésique célèbre

En 1950, le Dr Wilder Penfield, directeur de l'Institut neurologique de Montréal, accueille donc Brenda Milner avec le scepticisme commun aux médecins de l'époque. Hebb avait déjà tenté de faire entrer la psychologie à l'Institut neurologique; le neurochirurgien et le psychologue avaient voulu prouver qu'un foyer épileptique pouvait endommager différentes fonctions du cerveau et que l'excision du foyer devait rétablir les fonctions endommagées. Le séjour de Hebb à l'Institut, où l'étude des patients opérés aux lobes frontaux n'avait rendu compte d'aucun changement des fonctions intellectuelles, n'avait évidemment pas convaincu le Dr Penfield. Le neurochirurgien continuait à croire qu'il pouvait établir lui-même théoriquement, sans recourir à la méthodologie expérimentale, les relations entre le comportement et le fonctionnement du cerveau.

Peu de temps après l'arrivée de Brenda Milner, un événement grave se produit: un des malades du Dr Penfield, opéré au lobe temporal gauche, devient amnésique à la suite d'une intervention routinière. Deux mois plus tard, la même chose se produit avec un autre patient, un ingénieur qui avait déjà été opéré, sans succès, puisqu'il continuait à avoir des crises d'épilepsie fréquentes.

« Il a perdu la mémoire après l'opération, raconte Brenda Milner. Son amnésie était antérograde, c'est-à-dire que dès que son attention était détournée, il oubliait ce qu'il venait de dire et de faire. Je me souviens qu'il nous avait dit: « Mais qu'est-ce que vous avez fait? » Cette question, il devait se la poser à tout instant. » Dans un rapport à la Société américaine de neurologie, Brenda Milner et Wilder Penfield formulent l'hypothèse qu'il doit y avoir une lésion occulte dans la même région de l'autre hémisphère.

À la suite de ce rapport, le Dr Penfield reçoit un appel d'un neurochirurgien de Hartford, au Connecticut, qui dit avoir la preuve tangible de ce qu'ils avancent. Intriguée, Brenda Milner se rend immédiatement sur les lieux. En 1955, elle rencontre H. M., qui deviendra son plus célèbre patient. Sous ces initiales se cache un jeune homme de 27 ans souffrant de crises d'épilepsie; leur fréquence avait atteint un point tel qu'il pouvait subir jusqu'à 12 crises mineures en deux heures et 50 crises majeures par année. À la suite d'une intervention chirurgicale du Dr W.B. Scoville de Hartford, ses crises disparurent, mais il souffrit d'une importante amnésie. Le chirurgien lui avait retiré, de chaque côté du cerveau, le noyau amygdalien et l'hippocampe, les organes les plus susceptibles d'être le siège de ces crises d'épilepsie.

L'amnésie de H. M. ne l'empêchait pas de parler et de s'émouvoir. Son quotient intellectuel avait même augmenté depuis l'arrêt de ses crises d'épilepsie. Mais ses souvenirs se sont subitement interrompus à 27 ans. Il ne peut se rappeler où il demeure, mais se souvient très bien de la maison de son enfance. Dès ses premières rencontres avec lui, Brenda Milner fait une découverte importante: la mémoire immédiate de H. M. est intacte. Il peut retenir une courte liste de chiffres, mais si son attention est détournée, par une sonnerie de téléphone par exemple, il oublie non seulement la liste, mais aussi l'endroit où il est et ce qu'il faisait. Par contre, il apprend, à une vitesse normale, à effectuer certains tests d'habileté. S'il oublie qu'il a exécuté le test en question, sa performance s'améliore néanmoins d'une fois à l'autre.

Cette découverte de Brenda Milner, qui prouve l'existence d'une mémoire des faits et d'une mémoire des habiletés, est déterminante pour la neuropsychologie. La mémoire ne peut plus être considérée comme un système global. Dorénavant, la psychologue va consacrer toute son énergie à mettre au point des tests et à analyser les données des malades opérés pour éviter que des accidents chirurgicaux ne se reproduisent. A partir de ce moment, la nécessité de la recherche en psychologie expérimentale et de ses applications cliniques est devenue évidente pour le Dr Penfield et les autres chirurgiens qui vont lui succéder.

Des tests pour comprendre le cerveau

À l'Institut neurologique de Montréal, Brenda Milner crée des tests qui permettent d'évaluer les différentes facettes de la mémoire et de situer le siège de la parole. De loin le plus spectaculaire, le test du sodium Amytal, mis au point par le neurologue japonais Wada, le Dr Rasmussen et Brenda Milner, permet d'endormir un hémisphère en injectant une drogue dans la carotide du patient. Le psychologue a alors environ dix minutes pour faire les tests qui vont déterminer les zones du langage et, peut-être, révéler des lésions occultes pouvant affecter la mémoire. Le jour suivant, le test est repris pour l'autre côté.

Au début des années 60, les recherches dérivées des tests élaborés avec l'aide du sodium Amytal ont permis à Brenda Milner de fournir les données statistiques définitives de la relation entre l'utilisation de la main et la localisation des zones du langage dans le cerveau, et de déterminer pour chaque individu à opérer, dans quel hémisphère se trouvent ces zones. Au siècle dernier, on avait constaté que les personnes atteintes à l'hémisphère gauche avaient souvent des problèmes de langage et on avait noté que la plupart étaient droitières. On a d'abord cru que pour les gens gauchers le siège du langage était situé dans l'hémisphère inverse, mais plusieurs neurologues ont constaté que la représentation de la parole chez ces personnes était plus difficile à repérer. Les recherches de la psychologue canadienne ont permis de déterminer qu'en effet, pour la grande majorité des droitiers (96%), la zone du langage est située dans l'hémisphère gauche, mais que, contrairement à ce qu'on pensait, pour 70% des personnes gauchères cette zone est située dans le même hémisphère et que, pour 15% de ces personnes, elle est répartie dans les deux hémisphères du cerveau.

« La plupart des tests utilisés pour étudier les lobes temporaux et frontaux en neuropsychologie, affirme le neuropsychologue Gabriel Leonard, ont été élaborés ici, à l'Institut neurologique de Montréal. Son Département de psychologie a été un pionnier, dans ce domaine, et reste l'un des meilleurs au monde. J'ai entendu parler de Brenda Milner pour la première fois pendant mes études à Dublin, en Irlande. Plusieurs la considèrent comme la fondatrice de la neuropsychologie. C'est certainement l'une des plus grandes. Elle n'a pas inventé tous les tests; certains viennent de la psychologie expérimentale et sont destinés à évaluer d'autres phénomènes. Par contre, elle doit les mettre au point et, bien souvent, les adapter comme ceux qui ont été utilisés avec H. M., et qui doivent être réduits en fonction de la capacité d'apprentissage de ce dernier et tenir compte de son amnésie. »

Aujourd'hui, il ne se fait pas d'opérations à l'Institut neurologique de Montréal sans que les psychologues, responsables de la clinique, ne soient mis au courant. « Nous voyons les malades avant l'opération, explique Michael Petrides, mais la plus grande part de notre travail se fait après la convalescence. Les tests psychologiques nous donnent beaucoup de renseignements sur le patient et, la plupart du temps, on peut déterminer l'hémisphère où se situent les zones du langage sans l'aide du test du sodium Amytal, lequel est utilisé le plus souvent pour les personnes gauchères. Pour des cas plus compliqués, Brenda Milner se rend dans la galerie de la salle d'opération pour répondre aux questions du chirurgien. » À sa façon, la psychologue guide le bistouri...

Si la contribution de Brenda Milner est plus connue dans la connaissance du fonctionnement des lobes temporaux en relation avec la mémoire, son élucidation plus récente du rôle des lobes frontaux est importante. On croyait, il n'y a pas si longtemps, que les lobes frontaux, particulièrement développés chez l'être humain, logeaient les plus hautes capacités intellectuelles et, pendant longtemps, les scientifiques ont tenté de déterminer le rôle de cette région du cerveau. Les lobotomies frontales pratiquées chez les personnes aliénées mentales n'ont pas permis de comprendre la fonction de ces zones, bien qu'on ait souligné des troubles de personnalité difficiles à analyser. Déjà, le Dr Penfield, qui avait opéré sa sœur souffrant d'une tumeur et lui avait enlevé une grande partie du lobe frontal, avait constaté ses problèmes d'organisation et décrit sa difficulté préparer un repas.

Au milieu des années 70, Brenda Milner a démontré le rôle des lobes frontaux dans l'organisation temporelle. Les patients qui ont des lésions à cet endroit ne sont pas amnésiques, mais éprouvent de la difficulté à se remémorer l'ordre des événements dans le temps; si on leur montre une série de trois photos, ils vont se souvenir de les avoir vues, mais ne peuvent se rappeler laquelle ils ont vue en premier. Les recherches de la psychologue ont aussi prouvé une certaine inflexibilité dans le comportement des malades; par exemple, ils ont beaucoup de difficulté à faire plusieurs choses à la fois. Ce qui explique que la sœur du Dr Penfield avait peine, entre autres, à surveiller le rôti tout en pensant aux légumes à préparer.

L'enfant chérie de la psychologie

Un congrès de neurologie à New York, une conférence à Londres ou à Marseille, un doctorat honorifique en passant, la vie de la plus célèbre neuropsychologue ressemble à un marathon sans fin. Même si elle se prête volontiers à des émissions de radio et à des entrevues avec des journalistes, c'est dans la communauté scientifique que Brenda Milner est la plus populaire. « Elle est une célébrité dans son domaine », affirme Laughlin Taylor, collaborateur de la première heure et directeur de la clinique de neuropsychologie à l'Institut. « Elle revient justement de New York où une partie de l'auditoire dut s'asseoir dans les couloirs parce qu'il n'y avait plus de place dans la salle! » La psychologue prépare ses conférences avec grand soin: elle élabore un scénario accompagné de diapositives sur lesquelles elle « improvise ». Par ailleurs, elle va jusqu'à vérifier la couleur des murs de la salle de conférence pour harmoniser ses vêtements...

Son souci de la perfection fait d'elle une chercheuse très exigeante pour elle-même mais aussi pour ses assistants. « Elle écrit très lentement, raconte M. Taylor; elle peut prendre une journée pour écrire une seule phrase, jusqu'à ce qu'elle soit satisfaite. « Je ne crois pas qu'il y ait un directeur de thèse plus exigeant qu'elle en Amérique du Nord, avoue pour sa part, Gabriel Leonard, qui semble avoir surmonté l'épreuve. Les mémoires sont réécrits plusieurs fois, jusqu'à ce qu'ils soient parfaits », ajoute-t-il. Ses étudiants l'ont surnommée le « Manchester filter », du nom de sa ville natale, mais la psychologue dit tenir ce goût pour la prose scientifique de son maître, Donald Hebb.

La notoriété du Département de psychologie de l'Institut neurologique de Montréal tient en grande partie à ce côté perfectionniste de sa directrice: la psychologie expérimentale se basant essentiellement sur la méthodologie et l'analyse des données, ainsi que la rigueur en garantissent la crédibilité. « Depuis quelques années, explique Brenda Milner, la grande explosion qu'ont connue les techniques d'imagerie nous permet de voir le cerveau comme on ne l'a jamais vu auparavant. Le développement de la technologie a rendu les disciplines de la neuropsychologie encore plus précises. »

« Pourtant, on ne sait pas encore ce qu'est la mémoire; plusieurs questions restent sans réponse. Les malades ne peuvent pas tout nous révéler. Il faut tirer des études faites chez les singes les informations qu'on ne peut vérifier chez l'être humain, en raison des limites des interventions chirurgicales. » La plus grande contribution de Brenda Milner est d'avoir approfondi les connaissances fondamentales sur le cerveau en prenant soin de les orienter vers des applications cliniques. C'est pourquoi, aujourd'hui, plusieurs patients lui doivent leurs plus beaux souvenirs.

WERNER ISRAEL

(October 4, 1931–   )

Killam Laureate, 1984

Werner Israel was born in Berlin in 1931. When he was five years old, his family emigrated to South Africa and settled in Cape Town, where his father struggled for a living, first as a travelling salesman, then as a book keeper. When his parents both fell seriously ill in the late thirties, Werner and his younger brother Peter were admitted to the Cape Jewish Orphanage, where they spent four spartan but not unhappy years.

His scientific interests were awakened early. Saturday evenings at the Orphanage were sometimes enlivened by astronomical slide shows provided by Jack Simenhoff, a high school teacher and keen amateur astronomer. These first impressions were reinforced by the quantitative data on planets and stars in an ancient set of encyclopedias which his father, seeing the way his son's interests were headed, had obtained from a pedlar in exchange for an old suit.

Soon Werner was haunting the South African Public Library every other day and poring over every astronomy book available. Hoping to understand how anyone could possibly assert with confidence that the temperature at the centre of the sun was 40 million degrees, he dipped into Eddington's "Internal Constitutions of the Stars", only to run into what seemed at first a hopeless barrier: mathematics. With the help of Silvanus P. Thompson's hoary old classic "Calculus Made Easy" and its encouraging motto: "what one fool can do, another can", even this did not prove insurmountable.

Werner enrolled at the University of Cape Town in 1949. Since the University offered no courses in astronomy, he elected to major in physics and mathematics. Among his first year lecturers, one stood out. Samuel Skewes, Senior Lecturer in Pure Mathematics, also a rugby coach and sometime warden of one of the student residences, was a creative analytic number theorist. He had given the first estimate— $10^{10^{34}}$, the largest number ever encountered in a mathematical problem—for what became known as the Skewes number, the lowest number at which $N/\ln N$, Gauss's approximation for the number of primes smaller than N, changes from an over- to an underestimate. Skewes' lectures did not give the usual cut-and-dried impression. He followed no text, but recreated the subject in his own way, often extemporizing as he went along. Of course this approach was not universally popular, and Skewes' first-year class—250 strong—could sometimes give him a hard time. But, for the attentive student, he offered unrivalled insight into the way a live mathematician really thinks in putting a mathematical structure together. On Werner it left an imprint afterwards reflected in his own teaching style. Years later in Canada, he was to recall wryly those early days when an engineering student in his second-year calculus class complained, very tactfully: "Sir, this textbook is no use. It doesn't follow your lectures."

As Werner was nearing the end of his second year of studies, his father suffered a fatal heart attack, leaving his widow to manage a marginally profitable and undercapitalized hardware store in one of the poorer sections of Cape Town. Although Werner managed to complete his B. Sc. , while working half-days in the store, it became clear that the business needed full-time attention and the prospect of further study receded. For two years he spent all his time in the shop, with no sign of an upturn in business. Finally his mother made a tough decision. No matter what the sacrifice, she would send him back to his studies. As she recalled later: "When I noticed that—after two years—he was still going to the back to wash his hands each time he weighed a pound of nails, I could see he wasn't cut out to be a hardware salesman." So somehow a manager was hired for the store, and Werner enrolled for a M. Sc. in Applied Mathematics. He managed to complete the requirements in one year, and was awarded a £400 scholarship for overseas study. But the family was still hard pressed financially, and an opening for a temporary Lecturer at the University was too good to pass up.

After two years, with the scholarship option about to lapse, he began to think seriously of applying for graduate work. A subject which fascinated him was the unified field theory which Einstein had been engaged on in the last years of his life. From the vantage point of Cape Town, this appeared to be at the very frontiers of current research. He could not know that it was at the time a rather unfashionable area of physics, and perhaps it would have made no difference if he had known. Einstein had died just the year before, but papers by Schrödinger, who had constructed a very similar theory, were still appearing in the Proceedings of the Royal Irish Academy.

Werner accordingly wrote to Schrödinger at the Dublin Institute for Advanced Studies, and in due course received a reply from John Lighton Synge, the new Director of the Institute, explaining that Schrödinger had retired to Vienna a few months earlier and had, moreover, become increasingly discouraged with unified field theory in the last year or two. However, Synge added, if Werner wished to work with him on general relativity, he would be welcome. Werner accepted this generous offer without hesitation. Synge was a name he had run into many times in the literature, most recently as the author of a remarkable but baffling paper which for the first time probed the complete interior of what we now call a Schwarzschild black hole.

The Dublin Institute did not normally admit research scholars from overseas unless they were already in possession of a doctorate. However, they were persuaded to make an exception in this instance (aided, no doubt, by a warm letter of reference from Skewes), and even offered a very welcome supplementary scholarship.

Werner arrived in Dublin in the fall of 1956. He counts this as the luckiest turn of events in his life—there he met Inge Margulies, also German-born, whose family had found refuge in Ireland just before the war. They were married in 1958, and have a daughter and a son.

The School of Theoretical Physics of the Institute was then in the care of two remarkable men, very different in background, temperament, and philosophy, yet bonded by their deep insight and attachment to classical analysis and the Hamiltonian tradition of mathematical physics. Cornelius Lanczos—Hungarian, mercurial, an excellent pianist who had played chamber music with the cellist Feuermann, and possessor of a sharp and sometimes mischievous wit that could send a room into convulsions—had worked with Einstein and was deeply, almost mystically, devoted to Einstein's dream of a unified theory, though not necessarily to Einstein's specific approaches to it. Synge—nephew of the great Irish playwright, and cousin to the Nobel prize-winning biochemist of the same name—was the more deliberate and affected to be the more pragmatic of the two. His goateed patrician appearance had once caused a passer-by he encountered near the Royal Festival Hall to raise his hat with the greeting "Good evening, Sir Thomas". (He does indeed look like Sir Thomas Beecham.) For a number of years he held the Chair of Applied Mathematics in Toronto. His daughter Kathleen is a brilliant mathematician at the Courant Institute in New York who married the Canadian composer Oscar Morawetz.

At that time the School occupied the upper floors of an old Georgian house overlooking Merrion Square. Tea was served in the library, which had a strategically placed blackboard. The intellectual thrust and parry between Synge and Lanczos was a joy never to be forgotten. A newcomer stood in awe as the chalk changed hands and new mathematical developments unfolded before his eyes. It was quite common for these discussions to continue till it was time to break for lunch.

Synge at this time was engaged in writing the monographs that were to revolutionize the approach to relativity theory. In a word, he demonstrated that relativistic thinking could be reduced to drawing pictures. Although it had been recognized from the beginning that Einstein's theory described a space-time continuum, it was universally felt that attempts to hone this into an intuitive tool were doomed by the complexities of curvature, four-dimensionality and, above all, the indefiniteness of the metric, which means, for instance, that the hypotenuse of a right triangle is actually shorter than the two sides if one of them is in a time-direction. By showing that it was possible to overcome these intuitive obstacles, Synge's books radically changed the old attitudes. The new geometrical style of reasoning was to reach full flower in the following decade when Roger Penrose and Stephen Hawking developed the topological insights which established that space-time singularities are a generic feature of the big bang and gravitational collapse.

As one of the first beneficiaries of this new way of looking at Einstein's theory, Werner worked on a number of problems in Dublin, thus planting seeds for much of his subsequent work. He experienced the thrill of having his first paper—

communicated by Synge to the Royal Society of London—accepted for publication in the Proceedings.

Stories of his absent-mindedness at this time (and later) are legion, though he swears most are apocryphal (and his wife swears they are true). One evening, coming home from a late session at the office, he stopped for a minute to telephone his wife. Then, rushing on to catch his bus, he noticed half-way down the street that his load of books seemed heavier than usual. It was too late to turn back. And so a pair of oncoming old ladies were startled to be handed a phone book with instructions to deposit it in the first kiosk they came to.

With his two-year term at the Institute drawing to a close, he began to scour the back pages of Nature for vacancies, and the name Edmonton caught his eye. Although Werner had no idea where Edmonton was on the North American map, the name was nevertheless familiar from his early forays into unified field theory. It was the address of Max Wyman, the relativist who had first obtained the complete spherically symmetric solution of Einstein's unified field equations by a tour-de-force of analysis.

Werner took up an assistant professorship in Mathematics at the University of Alberta in the Fall of 1958. Wyman was already being drawn into the administrative duties which eventually took him to the Presidency of the University, and the hoped-for collaboration did not materialize (at least not up till now), but his interest and encouragement were a vital inspirational force. Other colleagues who became friends were George Ford (Head of Mechanical Engineering), John McNamee and Lee Lorch in the Mathematics Department, and Harry Schiff, Lynn Trainor, Avadh Bhatia, and Don Betts in the Physics Department.

Those early years at Alberta were not easy. With teaching loads in Mathematics of 11 (in some cases, 15) hours per week, large classes, and no marking assistance (since there was no graduate faculty to speak of), little time or energy was left for research. These burdens gradually eased as the University expanded in the 1960's, and Wyman and other progressively-minded voices gradually convinced the adminstrators that research was something more than occupational therapy for professors.

Limited funds for travel began to be generally available. At a meeting in Purdue in the Fall of 1963, Werner had a chance encounter with a Welsh relativist, Ivor Robinson, who had settled in Dallas and was then organizing a conference to discuss a mysterious class of new astronomical objects which had come to light earlier that year. This resulted in an invitation to attend (with all expenses paid) what became known as the First Texas Symposium on Relativistic Astrophysics, a historic meeting at which the new objects were dubbed quasars.

Werner still counts this as the most inspiring conference he ever attended. It was here that he had his first glimpse of many of the luminaries of astronomy and theoretical physics. Many younger workers also attended, among them Roy Kerr—a New Zealander then completing his graduate work in Austin—who explained to Werner the properties of a new solution of the Einstein field equations he had worked out a couple of months before. These properties were so bizarre that Werner was convinced the solution could have nothing to do with any real physical object. Three years later, he was the first to conjecture, and to make an important contribution towards proving, that Kerr's solution was not only a possible, but was in fact the only possible external field of what was to become known as a black hole.

Sheer luck—being in the right place at the right time, or being the only person to have at his fingertips two disparate pieces of information or technique—can play a crucial role in scientific discovery. In 1975, a chance remark by a colleague, Brandon Carter, in a street-corner conversation near the Collège de France in Paris led to a formation (partly in collaboration with John Stewart in Cambridge) of relativistic thermodynamics which avoids the paradox of infinite propagation speeds for heat that had plagued the older theories. About the same time, attending seminars in Cambridge by Stephen Hawking and his group on black hole evaporation, Werner noticed a peculiar resemblance to an apparently unrelated seminar, on quantum field theory at finite temperature, given a few months earlier in Edmonton by his friend and colleague Yasushi Takahashi. The result was a paper on the "thermofield dynamics" of black holes, a subject still actively under development.

Another form of luck is to hit on a topic seemingly of pure academic interest that is found, years later, to have far-reaching applications in another part of science. A paper on thin shells of matter, quite properly ignored when it first appeared in 1966, became two decades later perhaps Werner's most cited publication, when it turned out to be exactly what was needed to understand the dynamics of expanding bubbles in Guth's inflationary theory of the very early universe. A similar paper on filamentary structures anticipated the intensive study of cosmic strings in the 1980's. In collaboration with his UBC colleague Bill Unruh, and Valery Frolov, a Soviet relativist, he has recently returned to this topic, and elucidated some of the strange features that arise when the gravitational field around a string becomes so strong that a black hole is formed.

His appointment, two years ago, as a Fellow of the Cosmology Program of the Canadian Institute for Advanced Research has left more free time to pursue problems of interest, and this is now bearing fruit. With one of his graduate students, Eric Poisson, he is currently looking at the inner structure of black holes, in particular, the compostition of the nucleus that lies at the centre of a black hole.

While he has received a number of honours, the award of an Izaak Walton Killam Prize in 1984 stood out as an event of special significance—not least because he was immediately deluged with invitations to join the Physics poker club.

Though he plays no instrument, he has a great passion for music, and cannot bear to be without it for long. A keen admirer of Angela Hewitt, John Eliot Gardiner, Louis Lortie, and many of the new generation of artists, he also enjoys returning often to his vintage recordings of Schnabel, Casals, Beecham, and the Busch Quartet. Apart from music and browsing tirelessly in second-hand bookshops, his work is his constant and most consuming hobby.

PIERRE DANSEREAU

(October 5, 1911 –   )

Killam Laureate, 1985

À la source de l'écologie[1]

À l'heure de l'éveil écologique, un homme, Pierre Dansereau, fait figure de précurseur. Reconnu mondialement pour ses travaux sur le dynamisme des forêts et les structures de végétation, il a offert une nouvelle vision de l'écologie végétale en l'enrichissant des notions empruntées, entre autres, à la géologie, à la géographie, à la géomorphologie, à la climatologie. Et, sur ce chemin périlleux, il a bâti un pont entre les sciences naturelles et les sciences humaines en appliquant les concepts écologiques à l'environnement rural et urbain de l'homme moderne.

Pierre Dansereau est sans doute l'un des rares scientifiques québécois à avoir une réelle notoriété internationale. Ses nombreuses *migrations* lui ont permis d'enseigner à l'Université du Michigan, à l'Université Columbia, au New York Botanical Garden, où il fut sous-directeur et chef du Département d'écologie, à l'Université de Puerto Rico et à l'Université de Lisbonne, au Brésil, en Nouvelle-Zélande, en tout, 20 universités sur 5 continents. À Montréal, il a enseigné dans toutes les universités, se liant finalement à la plus jeune, l'Université du Québec à Montréal où, à 79 ans, il dirige avec enthousiasme ses étudiants, poursuit ses recherches et rédige sa prochaine synthèse sur l'écologie: *Ecological Impact: Range and Responses.*

Sur le terrain, il a fait appel à des chercheurs de toutes disciplines, qui représentent aussi bien la taxonomie végétale que la psychologie, la géologie ou la sociologie, donnant ainsi un sens véritable au mot « multidisciplinaire ». Mais celui qu'on a appelé « l'écologiste aux pieds nus » a non seulement tenté de faire une synthèse des connaissances scientifiques, mais a utilisé tous ses sens à la compréhension de la biosphère.

« C'est au niveau du paysage, écrit-il, que la véritable perception s'exerce, que la relation peut être décelée, que l'échange entre l'environnement et l'organisme peut être vu: transpiration, enracinement, croissance, floraison, dispersion, tombent tous sous le coup de l'observation. Tous les sens avisent l'observateur, surtout s'il voyage à pied, des différences dans la couverture végétale: l'air humide et frais sous le dais des épinettes et la chaleur sèche d'un champ avoisinant de verges d'or, le feuillage sombre et les émanations sulfureuses dans le marécage de quenouilles. »

« Pierre Dansereau, explique son ami, l'écologiste André Bouchard, est un homme qui a tendance à présenter les choses dans une très grande complexité. Devant une situation apparemment simple et qui amène un jugement simple, il aime inonder les faits d'autres implications pour montrer que, tout compte fait, ce n'est pas si simple que ça et qu'il faut bien voir les enjeux avant de prendre des décisions. Ce petit côté provocateur lui a gardé une jeunesse d'esprit. »

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[1] Cette biographie est adaptée du texte tiré du livre *Une Passion: la SCIENCE: Portraits de pionniers québécois* par Claire Chabot (Éditions MultiMondes, 1990), avec permission.

L'héritier du frère Marie-Victorin

Rien ne destinait Pierre Dansereau à une carrière scientifique. Il faut dire que le cours classique dispensé par les jésuites ne favorisait en rien la mise en valeur de ses talents de scientifique. La littérature et la contestation politique ont été ses premiers faits d'armes.

À la tête des Jeune-Canada, avec son camarade André Laurendeau, il publie, en 1932, le *Manifeste de la jeune génération*, prononce des discours incendiaires et nationalistes sur les injustices sociales et les trusts. « Nous avons été vilipendés par le premier ministre de la province de Québec, M. Taschereau, qui a dit de moi: « Ce jeune homme porte les encycliques dans une main et du vitriol dans l'autre. » »

Délaissant l'étude du droit, le jeune Dansereau décide qu'il sera fermier. Il s'inscrit donc à l'Institut agricole d'Oka, rattaché à l'Université de Montréal, pour y acquérir les connaissances nécessaires à l'exploitation d'une ferme. L'étude de la botanique est une révélation; les sciences naturelles, oui, mais plus question de vivre sur une ferme.

Le frère Marie-Victorin lui sert de modèle: « Il se penchait sur les plantes et avait réussi, avec une œuvre monumentale comme la *Flore laurentienne*, à nous placer enfin sur la carte du monde, dans le grand concert de la biologie contemporaine. Nous avions un indigène de notre culture qui tirait une œuvre de lui-même et du paysage qui l'entourait. Alors moi, j'ai décidé d'aller dans cette direction-là. »

Cette influence profonde du grand botaniste ouvre à Pierre Dansereau des horizons nouveaux; l'intérêt du frère Marie-Victorin pour l'évolution, le dynamisme de la végétation, la génétique et la distribution géographique des plantes oriente le jeune étudiant vers l'écologie. En 1936, il part poursuivre ses études en Europe. Mais une déception l'attend: l'écologie n'est pas au programme, il devra faire son doctorat en taxonomie végétale.

« Dans les années 30, raconte Dansereau, il ne se donnait pratiquement pas de cours d'écologie. Seules quelques universités du Mid-West américain et peut-être de l'Allemagne acceptaient cette nouvelle science. En marge, il y avait un grand écologiste, J. Braun-Blanquet, qui dirigeait une station de recherche à Montpellier. J'aurais bien voulu y faire mon doctorat, mais il avait été exclu des structures universitaires. J'ai fait mon doctorat à l'Université de Genève, reconnue pour ses contributions en taxonomie, tout en gardant le contact avec les écologistes de l'époque. »

Pourtant, les débuts de l'écologie remontent au milieu du *XIX*^{e} siècle. Cette nouvelle science, issue de la botanique, de la zoologie et d'autres disciplines connexes, tentait de comprendre le fonctionnement des plantes et des animaux dans leur milieu. Les premiers écologistes ont étudié l'impact du climat, du sol, de la nutrition, puis l'interrelation entre le monde végétal et le monde animal.

« On a tout de suite distingué deux types d'écologie, explique Dansereau: dans le premier, on étudiait, par exemple, un castor. On s'intéressait à sa nourriture, à l'impact que ses barrages produisaient sur la végétation aquatique. Une eau courante et une eau stagnante, c'est deux milieux complètement différents. L'écologie du deuxième type consistait en l'étude d'un milieu: une tourbière, ou une forêt par exemple. » C'est cette partie de l'écologie qui étudie les relations entre le sol, les plantes, les animaux, pour comprendre la cohabitation des espèces, les cycles naturels, leur équilibre et leur succession, qu'a développée Pierre Dansereau.

Un précurseur « dérangeant »

De retour au pays en 1939, Pierre Dansereau s'associe au personnel du Jardin botanique où il côtoie le frère Marie-Victorin, Jules Brunel, Jacques Rousseau, Ernest Rouleau et Marcel Raymond. Déjà, il se distingue de ces grands botanistes par son intérêt résolument écologique. « J'ai donné le premier cours d'écologie à l'Université de Montréal, mais on me gardait en marge du personnel enseignant, se souvient M. Dansereau. C'est à cette époque que j'ai commencé mes recherches sur l'érablière laurentienne. »

« Dès ses débuts, Pierre Dansereau pratiquait une écologie remarquablement différente; tandis que les écologistes de l'époque se contentaient de faire la description des associations végétales, il se servait de ces méthodes pour décrire les changements évolutifs et les successions, explique André Bouchard, conservateur du Jardin botanique de Montréal et professeur titulaire d'écologie à l'Université de Montréal. L'étude de l'érablière laurentienne, l'une de ses plus grandes contributions, a dérangé les écologistes de l'époque.

Les voyages de l'écologiste à travers les cinq continents, les recherches sur des terrains aussi éloignés par leur géographie que leur écologie (de la Terre de Baffin aux forêts du Brésil, de la Nouvelle-Zélande aux Açores) nourrissent sa vision de l'environnement. Il propose alors un système unique, que certains appellent le système Dansereau, qui décrit les structures végétales à partir de leurs dimensions spatiales, sans tenir compte de leurs affiliations taxonomiques.

Ce système permet de comparer des structures de végétation à travers le monde (des arbustes d'aubépines du Québec et des acacias des plateaux africains) et de faire un parallèle entre les contrôles biologiques des deux régions étudiées. « Ce système a même intéressé le U.S. Army Corps of Engineers, mentionne Pierre Dansereau, pour la détection stratégique des masses végétales. » « C'est un système dont on ne se sert pas beaucoup, aujourd'hui, à cause de ses difficultés d'application, explique André Bouchard, mais il va garder une position permanente dans les écrits scientifiques. C'est un nouveau jalon que Pierre Dansereau a posé dans le développement de l'écologie. »

« À la fin des années 40, raconte Pierre Dansereau, je poursuivais mes recherches tout en dirigeant le Service de biogéographie de la province de Québec. C'était un milieu idéal pour la recherche, mais le Gouvernement me donnait un salaire de famine: 2 000 $ par année. À l'Université de Montréal, on ne voulait pas de moi; on refusait de m'offrir un poste de professeur à temps plein. »

L'écologiste, se sentant pris dans un cul-de-sac, part aux États-Unis enseigner à l'Université du Michigan. Il découvre l'ouverture d'esprit des universités américaines et une pédagogie plus souple, plus à son image. Ces cinq années lui donneront un cadre idéal pour poursuivre ses recherches et mettre au point des méthodes d'enseignement modernes: utilisation de l'audiovisuel, de cartes et de photos, et recherches sur le terrain.

C'est à cette époque que Pierre Dansereau écrit son œuvre majeure: *Biogeography, an Ecological Perspective.* Devenu un classique en son genre, ce livre présente une synthèse des sciences écologiques. « Ce qu'il y avait d'original, explique M. Bouchard, c'était l'ampleur disciplinaire et géographique. Il s'agit d'un des tout premiers volumes à inclure un champ aussi large de disciplines scientifiques. La principale caractéristique de Pierre Dansereau est justement d'avoir des connaissances remarquables et d'avoir réussi à en faire la synthèse. »

Pour la première fois, l'écologie végétale va s'étendre à l'homme et l'inclure dans l'écosystème. « Il est difficile d'assigner à l'homme une niche bien taillée dans l'ordre naturel des paysages qu'il occupe, écrivait, en 1965, Pierre Dansereau: beaucoup plus que les termites, les castors et d'autres animaux, il a le pouvoir de faire dévier les forces du paysage, même si l'effet de son intervention n'est pas toujours d'en augmenter le rendement. Quant à l'homme, son histoire en est précisément une d'émancipation vis-à-vis du climat et même de la distribution des ressources. Sans en venir à l'homme urbanisé ni même à l'agriculture mécanisée, on ne peut que constater les énormes progrès faits par notre espèce grâce à une domestication de la végétation naturelle. »

L'écologiste aux pieds nus

En 1955, Pierre Dansereau effectue un retour au Québec quasi glorieux: d'assistant-professeur qu'il était, il devient doyen de la Faculté des sciences de l'Université de Montréal. Le nouveau doyen reprend le flambeau du militantisme en fondant le Rassemblement, qui réunit les Pierre Trudeau, Gérard Pelletier, Jean Marchand, Jacques Hébert, André Laurendeau et Jean-Paul Lefebvre. L'écologiste combat avec ferveur le régime de Duplessis, mais ses interventions politiques lui attirent bientôt les foudres des administrateurs de l'Université de Montréal.

« Il n'y avait pas que mes idées politiques qui dérangeaient, précise M. Dansereau. Quand je suis revenu des Etats-Unis, j'ai voulu appliquer mes idées pédagogiques à l'Université, mais je me suis heurté à la volonté des professeurs en place de maintenir la tradition, de rester dans leur tour d'ivoire. À la différence des Américains, ici, entre le professeur et les étudiants, on se lance des « Monsieur », et on se contente, plus souvent qu'autrement, de donner des cours magistraux. » Devant cette incompréhension mutuelle, le doyen quitte son poste et s'expatrie pour la deuxième fois aux États-Unis. Il laisse derrière lui des étudiants voués à l'écologie, mais aussi la certitude que les soucis administratifs ne conviennent pas à sa personnalité d'anarchiste.

Heureux de retrouver sa liberté, il continue ses recherches sur le terrain, se mouillant les pieds pour vrai, cette fois-ci! Ses convictions pédagogiques, il les clame dans ce qu'il aime appeler son « sermon sur la montagne ». À l'occasion de l'inauguration de l'Institut de recherche arctique et alpine de l'Université du Colorado, il prononce un discours à la gloire des « scientifiques aux pieds nus ». « Il y avait dans l'assemblée les plus hauts représentants de la recherche moléculaire, des scientifiques de laboratoire », évoque l'écologiste, le sourire aux lèvres.

Au moment de la Révolution tranquille, Pierre Dansereau devient sous-directeur du Jardin botanique de New York, situé dans le Bronx. L'observation de cette mégalopole lui fait franchir le pas vers l'écologie humaine. Les quartiers surpeuplés, la pollution et les espaces verts délimités lui font voir avec plus d'acuité la nécessité d'inclure l'homme dans l'étude environnementale.

« Un jour, une dame très riche est venue me voir au Jardin botanique. Elle nous offrait une somme considérable pour l'organisation d'un symposium sur les fleurs sauvages. Je lui ai dit: « Regardez autour de vous, qu'est-ce que vous voyez? La pollution de l'air et de l'eau, des constructions et des routes mal planifiées. Ce qu'il faut organiser, c'est un symposium sur ces problèmes urbains. » Ce qui fut fait et l'événement fut un succès! Pour la première fois, un symposium scientifique s'ouvrait aux représentants de l'industrie privée et de l'administration publique. Le dialogue venait de s'engager entre les scientifiques, les industriels et les administrateurs publics.

Les premières tentatives des planificateurs publics pour inclure des recherches écologiques dans l'étude des grands projets d'exploitation du territoire remontent au début des années 70. Une des premières recherches dans le monde, visant à mesurer l'impact humain sur l'écosystème touché par un projet d'envergure, à l'occasion de la construction de l'aéroport international de Mirabel, a été conduite par Pierre Dansereau. « Naturellement, dit le chercheur, nous n'avons pas pris part aux décisions, puisqu'elles avaient déjà été prises au moment où commençait notre recherche, mais nous avons tout de même essayé de limiter les dégâts. »

« Mirabel, explique André Bouchard qui a participé à cette étude, a été l'un des premiers essais de multidisciplinarité en écologie au Québec; il y avait des chercheurs des sciences naturelles et des sciences sociales, des anthropologues et des psychologues. Mais le peu de temps laissé aux chercheurs pour réaliser leur étude a empêché le projet de devenir une école de pensée. Les chercheurs de disciplines différentes doivent harmoniser leurs méthodes de recherche sur le terrain. Il aurait fallu continuer cette étude pendant une décennie. La contribution de M. Dansereau à cette recherche est importante: elle a donné une vision globale de l'écologie et de son insertion dans les sciences sociales. Je suis convaincu que les projets suivants ont puisé leur inspiration dans cette vision nouvelle de l'écologie. »

« D'une certaine façon, explique pour sa part Jean-Guy Vaillancourt, professeur de sociologie à l'Université de Montréal, le projet ÉZAIM (Écologie de la zone de l'aéroport international de Montréal) aura légitimé l'importance de faire des études d'impact. Aujourd'hui, on ne peut plus réaliser de grands projets d'aménagement sans mener de telles études. Il y a même des projets qui sont morts dans l'oeuf parce qu'on savait que l'étude d'impact ne les justifierait pas. Mais l'intérêt du projet ÉZAIM, pour moi, comme sociologue, c'est ce pont que Pierre Dansereau a construit entre ce qu'il appelle « les deux solitudes » , les sciences biologiques et humaines. Et il avait les connaissances et la crédibilité pour le faire » , ajoute M. Vaillancourt, qui s'intéresse de très près aux questions d'environnement.

L'écologie: pour un nouvel humanisme

Pierre Dansereau a passé les dix dernières années à tenter de faire la synthèse des connaissances en écologie. Ses réflexions ont donné un premier système permettant d'analyser différents écosystèmes, dont celui de Mirabel. Illustré par une boule de flèches, le schéma représente six niveaux trophiques connus: la *minérotrophie*, qui englobe des phénomènes comme l'érosion, la sédimentation et l'évaporation; la *phytotrophie*, l'exploitation du sol, de l'air et de l'eau par les plantes; les *zootrophies herbivore et carnivore*, qui suivent le modèle de la chaîne alimentaire; l'*investissement*, par exemple, les rues et objets produits par l'homme; le *contrôle*, c'est-à-dire le pouvoir que détiennent certains agents tels que le castor ou, le plus connu, l'homme.

C'est à ces deux niveaux supérieurs que Pierre Dansereau a vraiment innové. Le système qu'il a élaboré permet d'étudier des milieux aussi différents qu'une érablière, un verger, une fromagerie, une épicerie ou une église. Dans chaque milieu, son schéma montre l'interaction entre les différents niveaux trophiques, et aussi l'influence extérieure qu'il exerce ou subit.

Les travaux actuels de l'écologiste portent sur un système de classification des paysages qu'il a élaboré à l'aide de cartographie en couleurs. « Il s'agit d'une méthode d'analyse spatiale qui permet de répondre à certaines questions, explique Pierre Dansereau. Par exemple, comment caractériser les différents espaces verts? Dans le secteur d'Outremont, où j'habite, il n'y a presque aucun espace vert public, mais 60% d'espaces verts privés. Alors, où est le besoin et comment y répond-on? »

L'intérêt de Pierre Dansereau pour la gestion écologique de l'environnement l'a amené à développer sa propre philosophie qu'il appelle « l'austérité joyeuse » . « Un partage des ressources, écrit-il, qui permet leur renouvellement à relativement court terme et qui assure au moins une gratification minimale des besoins de l'homme n'est compatible, ni avec le présent rythme auquel la population croît et celui où les ressources sont exploitées, ni avec l'allocation actuelle des pouvoirs de les utiliser et de les accumuler. »

« On n'a pas besoin d'adhérer à une idéologie politique en particulier pour reconnaître le désordre engendré par la compulsion sociale actuelle d'acheter et d'étaler, de surconsommer et, éventuellement, de gaspiller. On n'a pas à se demander si cela est moral, si cela est responsable. On peut se poser la question plus pragmatique: « Est-ce que ça peut continuer? » La preuve nous est constamment fournie que les riches deviennent plus riches et les pauvres plus pauvres. Mais il semble bien que même un système de répartition plus juste ne résoudra pas nos problèmes à long terme et qu'un ralentissement de la production et de la consommation sera bientôt considéré comme aussi essentiel que la réduction elle-même du rythme d'augmentation de la population humaine. Ceci réclame des sacrifices et une certaine austérité. »

« Dans mes moments les plus optimistes, ajoute-t-il, je crois que l'écologie est la base d'un nouvel humanisme et que l'on doit écrire l'histoire écologique, et non plus l'histoire linéaire, militaire, économique. Il faut reprendre le fil du devenir de la France, de l'Angleterre, des États-Unis en essayant de définir quels usages les nationaux ont fait des ressources qui leur étaient accessibles. Quelles mesures ont-ils prises pour s'approprier celles de l'Afrique et de l'Amérique du Sud, le Tiers-Monde actuel. »

« Peu de scientifiques, mentionne Jean-Guy Vaillancourt, n'ont pas peur de prendre position sur des questions politiques et sociales, de se metre les pieds à l'eau. À plusieurs reprises, Pierre Dansereau s'est mis sur la ligne de feu, politiquement. Ces dernières années, il fait un travail de cartographie servant à la réforme agraire au Nicaragua. C'est courageux d'y aller et d'appuyer ce qui se fait là-bas. »

Pierre Dansereau n'est pas un bâtisseur d'empire. Il n'a pas fondé d'institut ni dirigé de département avec une poigne de fer. Mais si, tout au long de sa carrière, on avait donné à cet humaniste tous les pouvoirs en matière de pédagogie, l'Université aurait mieux bénéficié de ses idées avant-gardistes. Son amour pour la langue française, qu'il écrit et parle si bien, ne l'a pas empêché de manifester une grande tolérance envers ceux qui n'ont pas ce talent. À l'Université Columbia, il a même été jusqu'à suggérer qu'un étudiant, particulièrement éloquent, soutienne sa thèse de doctorat devant une caméra, au lieu de l'écrire. Inutile de dire qu'on a trouvé sa suggestion des plus excentriques...

En 1987, Pierre Dansereau explorait les îles Galapagos, sur les traces de Darwin. Toute sa vie, cet écologiste a vécu des contacts intenses avec la nature et l'homme. Au moment où ils se préoccupent de leur environnement, les Québécois découvrent la portée de l'œuvre scientifique de Pierre Dansereau. Ils s'aperçoivent soudain que, depuis près de 50 ans, cet homme marque l'histoire de l'écologie.

PHIL GOLD

(September 17,1936–)

Killam Laureate, 1985

1. Birthplace and family details

I was born in Montreal, September 17, 1936, the son of Rose and Jack Gold. Both of my parents had immigrated to Canada from the Polish area of the Pale of Settlement in the 1920's. My father, who had apprenticed as a tailor in Europe, continued his trade in Montreal and developed into a fine designer of women's fashions before retiring. I mourn his recent passing. Mother raised a family of two, my sister and myself, and has remained a housewife to the present day.

2. Early education and influences

I was educated in Montreal at Bancroft Grade School and Baron Byng High School. The first major influence on me was the realization that what was being taught in fifth grade was very reminiscent of what had been taught in fourth grade, so I obtained permission from the school Principal to spend the next three years, for the most part, in the school library. During that period of time, I learned a great number of things, the most important of which was that enormously important "secrets" of nature, history, the sciences and humanities could be gleamed from simply reading books. This process continued through High School, where I gained the distinct impression that the teaching system was aimed at the "middle third" of the class (however that was determined), leaving the top third and the bottom third thoroughly bored.

Upon arriving at McGill University, I decided to opt for the sciences and, ultimately, for the biological sciences and Honours Physiology. During that period of time, major influences on me were Prof. Frank C. McIntosh, the co-discoverer of acetylcholine, as a neurotransmitter; Prof. Ben Burns, a neurophysiologist who did yeoman work in the delineation and physiology of the Respiratory Centre and, most importantly, Prof. Arnold Burgen (presently Sir Arnold Burgen), who in recent years has been President of the Medical Research Council of Britain and the Master of Darwin College, Cambridge; and is now President of Academea Europea, the newly formed "Royal Society of Europe". Prof. Burgen became my mentor and friend with whom I did my master's Thesis during the summer periods of my medical school years (which were yet to come). His genius made an impact upon me which will certainly last for the rest of my life. He was, indeed, the first true genius I had ever met and it may be that he is the only one that I will have known well.

Upon completion of Honours Physiology, I entered the Medical School at McGill University but continued in my research activities toward a Master's Degree in the field of control of red blood cell production by the hormone Erythropoietin. My Master's thesis was completed simultaneously with the completion of my medical schooling.

During the four years of medical school, I continued to acquire the information available in biomedical science and the application of this information to the care of patients. It became patently obvious to me, during that time, that although a great deal of information had been acquired vis-à-vis the basic parameters of biology, in reality only a few small steps had been taken on the long and convoluted trail that would lead to a comprehensive knowledge of the parameters underlying life processes and nature in general.

Medical School was followed by a year of Rotating Internship and a first year of Residency in General Internal Medicine in the McGill system at The Montreal General Hospital. During those two years, I had become increasingly aware of the burgeoning field of Immunology and the potential that it held, both technologically and conceptually, for addressing a number of questions that were basic to the process of the maintenance of cellular function, recognition, and differentiation. In addition, I had become increasingly interested in the mechanisms of malignant transformation of the normal cell during the process of cancer development. It was an attempt to utilize immunologic technology in order to address the cancer problem that led me to initiating my Ph.D. work in the biology of the cancer cell and resulted in the discovery of the carcinoembryonic antigen (CEA) during the two years in which I completed my Ph.D. thesis. This discovery ultimately led to the first widely used blood test for cancer and a major new understanding of cancer cell biology.

At the completion of my Ph.D. dissertation, I returned to the Residency Program in General Internal Medicine and obtained permission to complete this course of training in one additional year. This was followed by further training (one year) in Clinical Immunology & Allergy, at the end of which time I went to New York City, and the laboratories of the New York Public Health Research Institute (Dr. Samuel Dales) in order to obtain an understanding of cell biology and cellular ultrastructure.

During this period of my training, the major influence on me was that of Dr. Samuel 0. Freedman, my Ph.D. thesis Director and a superb Clinical Immunologist.

3. Range of interests and career possibilities as a student

I was immediately attracted to the biological sciences but I developed, and retained, a substantive interest in both the physical and earth sciences. Indeed, as time went on, the impact of the physical sciences upon the understanding of biologic processes became increasingly clear.

I would like to underscore that my interest in the sciences did not in any way preclude or diminish a love of the arts, including music, literature, philosophy, and dance. Indeed, it would seem that the prolongation of life would have little meaning if one could not enjoy what the arts and humanities have to offer.

At one point, I had seriously considered, and had applied for and received acceptances to schools of journalism prior to deciding on a career in biomedicine.

4. Beginnings of my interest in and commitment to Science

To a large extent, I have already indicated how I moved into science. I suppose that, if there was one particular reason for science as a career, it is perhaps the child-like desire to "know a secret that no one else knows". Throughout most of grade school, high school, and university (both in Honours Physiology and in Medicine) we seemed continually to be learning "the basics" I can't say exactly at what point the desire arose, but there was a time when I developed that need "to know something that no else knew" and, perhaps, to see something that no one else had yet seen and, perhaps, to think something else that no one else had thought (as Schopenhauer, might have put it). The influences of such individuals as Sir Arnold Burgen, whose phenomenal intellect I found almost staggering, unquestionably sharpened this desire.

5. Professional qualifications and experience

As noted above, I have had training in Physiology (which of necessity included a fair amount of mathematics, statistics, chemistry, physics, and biochemistry and other biological sciences), the various topics of Medicine — both basic and clinical and, in particular, basic and clinical Immunology and Oncology (the study of cancer). Professional qualifications include those in Medicine, the Ph.D. in Physiology (Immunology), a Fellowship in Internal Medicine and Clinical Immunology & Allergy, and a fair amount of experience in tissue culture, molecular biology, and cellular ultrastructure.

6. Unexpected twists of fate

For a brief time, I had given some consideration of doing my undergraduate degree in Geology. Being basically lazy, however, and learning that the field trips in Geology were all on Saturday mornings, I opted for the biological sciences which, on balance, held more interest intrinsically. My encounter with Arnold Burgen, who at that time had only recently arrived in Canada, was one of enormous importance to me. Hence, it is perhaps somewhat ironic that, when I first discussed my interest in pursuing a doctoral degree in the area of Cancer Immunology, Prof. Burgen was rather opposed to the concept and provided substantive, and in retrospect very appropriate, arguments as to why this was not the route to take from the point of view both of the problem being addressed and, perhaps, of the future of biomedical research.

As fate would have it, however, Dr. Burgen had been invited back to Cambridge University to take on the Chairmanship of the Department of Pharmacology and I had the good fortune to encounter Dr. Samuel 0. Freedman who, after some conversations, decided that the idea that I had in mind was "worth a shot".

7. Most exciting and most frustrating research experience (a realistic flavour)

I expect that the most exciting research experience I have had also proved the most frustrating. This was the discovery of the carcinoembryonic antigen (CEA). This was the "first secret no one else knew about" which, in virtual ecstasy, I prepared for publication in a journal from which I had been warned to stay away. This was the *Journal of Experimental Medicine* and the reason for the warning was that Peyton Rous (Nobel Laureate for his work in cancer virology) was on the Board of the journal and had little faith or trust in the immunological approach to the study of cancer cell biology. That admonition notwithstanding, the article was submitted to the journal and was accepted within a matter of two or three weeks. Indeed, a note, handwritten from Dr. Rous, appeared in my office shortly afterward with a single sentence on it that said "I believe it". This, if anything, added to my elation but the frustration was yet to come.

Before that period of time I had come to believe that the trilogy of biomedicine was that of 1) patient care; 2) teaching; and 3) biomedical research. Indeed, I now found to my distress that in the "real world" the trilogy actually runs: 1) if it's new, it's not true; 2) if it's true, it's not important; and 3) if it's new and important—then "We knew it all the time!". Indeed, it took almost a decade for the work, and all of the material that followed, to be appropriately reproduced in its various phases and applications. It was during that decade that I learned the importance of being *confident* in the results obtained in the laboratory. Only then is it possible to "hang in" despite the various "slings and arrows" that may be hurled by senior and respected scientists. Scientific truths will, eventually, be reproducible in good laboratories.

8. Hobbies, enthusiasms, passions, causes — life intertwined with sciences

In terms of hobbies, I tend to be a dilettante, moving from one to another with some rapidity. These have included, over the years, lapidary, gemology, the growth of various types of rare plants, the breeding of tropical fish, etc. Those which have remained, however, have been photography and allegiances to music, literature, fine art and dance, along with a number of other arts and humanities. Small craft sailing is an ongoing passion.

As regards causes, a great deal of my time has been spent in those functions related to the Jewish community in which I live, most particularly in Jewish education and propagation of Jewish culture. I have also been bound up over time with the cause of Russian Jewry and, most particularly, the cause of Soviet scientists who have lost their posts simply because of religious backgrounds and not necessarily because they wish to emigrate from the Soviet Union. Ongoing communication with these colleagues and supply of data to them has, I now know, helped them to withstand some of the difficulties that they encountered and, indeed, one of these colleagues has recently been elected an Associate Member of the National Academy of Science of the Soviet Union and has appeared at a variety of international meetings in the last five years because of ongoing pressure brought to bear by colleagues outside the U.S.S.R.

9. International contacts and activities

I have had the good fortune to be a member of a large variety of national and international organizations. Most important is the friendship that one has with individuals whose backgrounds vary a great deal scientifically, nationally, culturally, politically, and economically. Happily, science supersedes all of these boundaries and, indeed, is the great equalizer. Happily, this also allows one to find colleagues in virtually any part of the world for a day or evening of conversation, good fellowship, and exchange of ideas.

10. Inspirational influence in you life and work (people, ideas, moments of insight)

The major influences that control one's future activites are now known to occur quite early in life. Hence, there can be no doubt that the atmosphere within our home and community, as well a parental desire based on long traditions of education and learning were inculcated in me at a very early age. Books were ubiquitous in our home and I had the good fortune to have the opportunity to both speak and read Yiddish before I actually began to learn to read English in school.

A good deal of inspiration came from exposure to the classics during the last three years of Grade School which I spent in the library of the school; in retrospect, the foresight of the school's Principal in allowing such an "experiment" to take place in the early-to-mid 1940's took a great deal of courage on his part.

The influences exerted by such men as Sir Arnold Burgen and Dr. S.O. Freedman (currently Vice-Principal, Academic, of McGill University), have been outlined above. Most important, however, in the past three decades, has been the unwavering support and encouragement provided by my wife, Evelyn, a superb fine artist, in virtually every aspect of the activity in which I have taken part. It is she, primarily, who has taken the rather strange lifestyle that develops during a scientific career in stride and has filled the various gaps that may have otherwise occurred in our lives with imagination and panache. She has shared in both my elation and my frustration and has allowed me to maintain a relatively level balance under both sets of circumstances. Our children, Ian (28, a final year doctoral student in Philosophy at Princeton University), Josie (25, a graphic designer with a Baccalaureate in Fine Arts), and Joel (21, a senior at Brown University), have always kept reality in sight.

One other relatively strange occurence that is worthy of note is the fact that, on two successive evenings during the first year of my Residency Training in General Internal Medicine, I attended two lectures. The first dealt with the concept, then still relatively new, of Immunological Tolerance; on the the second night, the lecture dealt with the inability to clearly distinguish differences between normal and cancerous cells. I have no doubt whatever that the juxtaposition of these two lectures played a major role in the ultimate designs of the experiments undertaken to demonstrate the presence of the CEA molecule in human colonic tumor tissue that was absent from the corresponding normal tissue.

11. Hopes and concerns for the future of sciences in Canada

Anyone working in the sciences in this country must, by this time, be clearly aware that the support for scientific research provided by government is the lowest, per capita, of all the countries of the Organization of Economically Developed Countries (OEDC). It is certainly not a statistic in which either the government or the people of Canada can take pride. The scientific community of this country, because of the relatively small population in a relatively vast area, is itself small. Hence, a substantial lobby for increased funding for science activities has not really existed and that which has been in place has not been particularly effective in influencing our politicians who appear to have two primary objectives: the first, to be elected; and the second, to be re-elected.

Our governments have taken no initiative to utilize the scientific strengths of our country in a way that would encourage the younger members of our community to enter the sciences. Moreover, there has, until recently, been an enormous hiatus in moving universities and industries together for the mutual benefit of both and of the people of Canada. Even now, when this initiative appears to be on the rise, it is apparent to all concerned that the initiative was taken not because of the good that may come of it, but really out of political expediency. Indeed, even the "injection" of $1.3 billion from government into Science and Technology in 1988/89 that has recently taken place, is in many ways a sham in that a great proportion of these funds were siphoned off for the Canadian Space program before any of it found its way into the university and university-industry scientific areas—and then over a five-year period.

The government has shown virtually no foresight nor strategic planning vis-à-vis the sciences in the development of Canada as an international power. Indeed, the objective would appear to be that which I have come to call part of the "Canadiana Syndrome" where mediocrity is at a premium and "safe research" is funded. Few steps have been taken to protect the young who train for scientific careers in order to assure that they will be given a first opportunity at establishing their careers appropriately. One could almost predict that Leonardo da Vinci would not be funded by our many Science Councils because he would obviously be considered a "dilettante" whose ideas were "too high risk".

Having looked at the negative side of the issue, it is certainly to be hoped that present forces arising within the scientific community, the universities, and the government (at least in small pockets) will become a united force to push science forward in a clear-cut fashion with a policy in place. Indeed, in order to succeed, a contract would have to be established between government, the universities and industry in order to allow the fruits of science which have developed within the universities to move into the developmental phases and, ultimately, into the industrial areas where they can generate both employment for the population in this high-tech era of industrial evolution and give Canada its well-deserved high profile in the areas of science and technology internationally and move us away from the ever-dangerous "resource-based-only" economy.

We need not develop our blueprint to be consonant with either the American or Japanese models—which have been only partially successful despite the enormous expenditure of funds in those countries. Nevertheless, we would do well to look at the manner in which, at least Japan, has focussed its energies, if not entirely wisely, at moving into the 21st century in a leadership role in science and technology. I think that our country is at a point where this can occur, and ongoing and necessary pressure from within the scientific community upon government, coupled always with a well-informed public supporting the initiative, may well allow us to take our proper scientific place amongst the other developed nations of the world.

12. Comments or reflections on the importance of young scientists and engineers to the future of Canada

The life's blood of science and engineering is our young, while the life's blood of our country is the development of science and engineering, along with their related fields. However, we must bear in mind that a society that forgets its arts and humanities needs no science and technology. Hence, if there is to be an area of emphasis within the model described above, it must be that of encouraging our young towards careers in science and engineering.

It is often said that the universities do not do enough to stimulate this kind of movement. The fact is that, by the time individuals come to university, they are probably beyond the age where they can easily be influenced in their career trajectories. Hence, it will be necessary to restructure both our grade school and high school curricula in order to bring science, engineering, and technology to the attention of our children when they are very young in order to be able to hold their attention and direct them toward these areas in their university and subsequent, careers. Nor do we have the luxury of allowing this to happen over the long term. It cannot be underscored strongly enough that, if we do not enter the 21st century, a mere 9–10 years away, with our young people well aware of the need for the development of science and engineering in Canada, our country may well be out of the international "game" in terms of leadership in this area. Moreover, this is unquestionably going to be reflected in our economic status, as time goes on. Hence, if one were to be simultaneously both optimistic and pessimistic, it could be said that, if we do not adjust to the reality of the situation quickly, Canada may wind up being the country that funds its sciences best ***in its capacity as an underdeveloped nation***.

RALPH GORDON STANTON

(October 21, 1923 –   )

Killam Laureate, 1985

1. Family.

I was born on October 21, 1923, on a farm seven miles outside Lambeth, Ontario; Lambeth itself is six miles from London. I presume that a midwife was present at the event; certainly my younger siblings were all born on the farm with such help. My mother, Ida Maude Robertson, had taught for several years at a local public school; my father, Gordon Wyman Stanton, was a local farmer. The name Wyman had been handed down in the family for some time (my grandfather was Edgar Wyman Stanton) and my grandmother was horrified at my mother's insistence that I not be saddled with the name. However, mother was a strong-minded woman and she prevailed; I was named Ralph after her brother and Gordon after my father.

The Stantons originally came from upper New York state in the 1830's; I would like to think that they were late United Empire Loyalists, but I suspect that my great-grandfather was merely attracted by the offer of free land in Ontario. He settled in the Streetsville area originally, but homesteaded our present farm near Lambeth about 1856. He brought my great-grandmother, young Edgar (my grandfather, 1849–1933), and the infant James (1856–1952) in a waggon over corduroy roads to their new home. Edgar married a beautiful neighbour, Hannah Emily Sutton, from whom I learned, as a child, many facts about the early family history; unfortunately, I have forgotten most of them except for a few names like those of "Uncle Warren" and "Aunt Polly Post" back in New York State. Uncle James never married but worked on the farm actively until he was 92, and somewhat more leisurely after that. He was a survivor of the middle ages and believed that baths removed the "protective coating"; the only time that I can recall his taking a bath was when he fell off the plank that led across the creek to our barn and took an impromptu dousing in Kettle Creek. He was absolutely furious, especially since his brother, my grandfather, was standing on the bank convulsed with laughter. Uncle James was very proud of being active; I can recall an incident when, at age 92, he was busy arranging oat sheaves in the loft as we unloaded a waggon-load of oats into the barn; my father offered to come up into the loft to help him, but assistance was gruffly rejected as Uncle James acidly retorted that he was "quite able to throw these sheaves around, and you too if you come up here".

2. Early Education.

Our house was large originally and had then been extended when my father married so that both my parents and my grandparents had separate wings of the house. So I grew up in a three-generation family. In eighty years, quite a few books had accumulated in the house and I read everything that was available from "Uncle Silvio's Secret" to the "Poems of Tennyson", and from Martha J. Finley's novels to the "Tanglewood Tales" of Hawthorne. Fortunately, there were quite a lot of good literary works: I found the poems of Longfellow appealing, especially the *Saga of King Olaf*, and I read the Bible and Shakespeare thoroughly. The latter two had a great influence on me, since I unconsciously absorbed a regard for language and style that has always remained with me. The New Reference Encyclopaedia was also a fascinating work, although it often left one with curiosity about details that were omitted.

My mother taught me a lot of early school material at home, concentrating on reading, writing, 'rithmetic, and "nature study" (she had spent a summer at the Ontario Agricultural College in Guelph). So I was able to enter public school quite late; I began one April in the Senior Second Grade (now called Grade 4). The next year I did Junior and Senior Third (Grades 5 and 6). Our school was a single room, with about thirty students in 8 grades. This gave you the great advantage that you could listen in on whatever was going on in any grade. At least once a week, we had the excitement of an arithmetic match, or a spelling match, or a geography match; sides would be drawn up, the whole school would participate, and everyone would have a splendid (and educational) time. The school had a good library for a rural school; there were several score of books kept in a big wardrobe at the front of the room. I remember in particular a very large number of the works of Dickens as well as "The Coral Island". I was very lucky to have gone through school under the old Ontario curriculum in which much emphasis was placed on English Grammar; this helped me enormously when I later studied foreign languages. It also helped me to appreciate the English language; in my opinion, the modern de-emphasis of grammar is one of the most unfortunate of recent educational developments. How can one speak and write well if one does not have an accurate and precise feeling for the building blocks of language? English is such a beautiful language, such a magnificent vehicle for the transmission of ideas from age to age, that it is a pity to see it mangled by modern semi-literate journalists. Just in to-day's newspaper, I notice a professor referring to "persons being very subterfuge", a journalist stating that it is necessary "to chose something else", a financial analyst assuring us that yuppies "are lavishing their children", and a columnist referring to "selecting a criteria". I find quotations like these more offensive than flatulence and more painful than a toothache.

I grew up in the depression, and money was always in very short supply around the farm. We sold eggs for 10 cents a dozen; a dressed chicken for 60 cents; a 90-pound bag of potatoes for under a dollar. So everyone was very careful with pennies, and I still mend my own socks and turn up the sleeves of a jacket when it becomes frayed. Naturally, I stayed in our local public school for the Fifth Form (Grade 9); that was possible then, and I do not think that it was any disadvantage to me. However, for Grades 10, 11, and 12, I went to Lambeth Continuation School seven miles away. In Lambeth, we must have had fifty or more students, and there were two teachers. Finally, for Grade 13 I went to South Collegiate Institute in London. To a country boy, this was a very large place; there were twenty teachers.

Teaching has always appealed to me; when I was in public school, I wanted to be a public school teacher. At South Collegiate, I decided that I wanted to be a High School teacher. I was undoubtedly influenced by the extremely high calibre of the South teaching staff at that time. I learned a great deal from the Mathematics and Science teachers, but I think that I was most influenced by W. G. McNeill, who imbued his history classes with enormous fascination, and by Charles Burns who brought the Latin world to life for me. Mr. Burns increased my regard for form and the structure of language; he also made me regret that Greek was not available in the curriculum.

3. Early Career.

I suppose that my career began when I won a scholarship to enter the University of Western Ontario in 1939. To us, Western was simply "the university". There was a vague knowledge that there was some place called Toronto, but that was very far away indeed; when I actually attended Toronto later, it was still a five-hour drive.

The Mathematics Department at Western was very small: four people, together with one part-time lecturer who taught us geometry, including a knowledge of the "circumcised circle of a triangle". However, it was when I was at Western that I moved my sights up and decided to become a university professor. The mathematics taught at Western was neither very inspiring nor very difficult, but I found that the Western library opened up an entirely new world to me. For the first time, I was able to read the works of some of the great figures we had talked about in history. Indeed, in my first year at Western, I ploughed through the complete works of Racine, Corneille, and Moliere among the moderns; Aeschylus, Sophocles, Euripides, and Aristophanes among the ancients. I have always been enthralled by the written word, and have not fallen prey to the modern mania for sound. My house still does not contain a radio, a television set, or a stereo, and I regard walking around with a Sony machine as one of the greatest of modern perversions.

Two books, two professors, and one collector, influenced me greatly at Western. I was impressed by the John Davis Barnett collection in the Western library; Dr Barnett had apparently saved every book that he had ever encountered, and this approach was very attractive to my natural magpie instincts. I took a course on Latin American History from J. J. Talman and a course on Spanish from Frances Montgomery; these two courses kindled in me a keen interest in the Hispanic world. And we were very lucky in the fact that our first-year programme required a "Survey of English Literature"; the texts used were C. G. Osgoode's "The Voice of England" for literary history and McCutchon and Vann's "Anthology" for the actual works. My experience with this English course has convinced me that it is a tremendous service to students to give them a feeling for the panorama of English literature, for the development of the language, and for the creations of the great masters (no, I am not advocating a compulsory English course in University; most English departments now would probably use such a course to ride their own hobby horses and to give a detailed study of some second-rate contemporary purveyors of paperbacks).

I suppose that I ended up in Mathematics at Western because it was more attractive than Physics or Chemistry at the time. Our first-year lecturer in Chemistry was so dull that he once managed to lecture for a full hour and give exactly two lines of lecture notes. And one of our Physics lecturers was so disorganized that he would stop one lecture in the middle of a theorem on magnetism and start the next lecture in the midst of a discussion of current electricity. So I became a mathematician.

4. Mathematical Training.

Certainly, when I graduated from Western, I had no idea what research was about; I can perhaps be forgiven for this, since the Head of the Mathematics Department certainly had no ideas about research either. His closest contact seems to have been when, as he proudly told us, he went on an eclipse expedition and was sent behind a rock to call out, in a loud voice, the numbers from 50 down to zero, at one-second intervals, in case all of the expedition's clocks should simultaneously fail during the eclipse.

My first real contact with research came on graduation; I worked for a summer with Lionel Penrose, who was then Director of Psychiatric Research for the Province of Ontario. Lionel was absolutely brilliant, with one of the most original minds that I have ever encountered. I learned a great deal about Statistics from him, and my summer with him resulted in collaboration in three papers and one that I wrote myself, with his encouragement, on "Filial and Fraternal Correlations in Successive Generations". Later, Lionel returned to England, where he became a Fellow of the Royal Society and Professor at University College; his family has carried on his tradition of outstanding research. Many Canadians owe a great deal to his work here and his influence here.

For graduate work, I went to the University of Toronto; at that time (1944) it was really the only graduate faculty in Canada and the Mathematics Department had the enormous staff of twelve full-time people, many of them outstanding. I took courses from most of them, but was particularly influenced by Richard Brauer, who became my thesis supervisor; H. S. M. (Donald) Coxeter, who led me towards the boundaries of combinatorics with his course on finite geometric configurations; and W. J. Webber, who impressed me with his up-to-date knowledge of the latest developments in Analysis. Outside mathematics, I found the Toronto library a wonderful place; it was in 1944 that I was first able to read the works of people like Richardson, Fielding, Smollett, Sterne, and a host of others who moved from merely being people discussed in "The Voice of England" to being real literary creators.

Chance plays a large part in any life, and chance influenced me at Toronto. I had thought of taking a course in Italian, because so many operas are written in Italian. However, the Italian course was five hours a week and so some of the meetings conflicted with my mathematics lectures. So I enrolled in a one-hour per week course in Portuguese, given by Milton Buchanan. At that time, Buchanan was the only Canadian ever to serve as President of the American Modern Language Association, and he was both a very distinguished scholar and a distinguished bibliophile; from him, I learned Portuguese and I also came to know a little about the lore of old books. Most of the other students in the Portuguese course were dabbling a little in Portuguese as a supplement to their work in Spanish or History, and they gradually disappeared. At the end of the year, I was the sole student left, and Buchanan regaled me with all sorts of interesting tidbits about language and bibliophily.

This was 1945 and I had completed my M.Sc. degree; one morning the telephone rang at 8 a.m. It was Professor Buchanan, who explained that Dr Cody, the President of the University of Toronto, had just approached him with a problem. The Brazilian Government had given four Canadians scholarships to study for a year in Brazil and had only then realized that all four were Francophones; could the University of Toronto please supply an anglophone to take up a fifth scholarship? I was rather timorous about spending a year in Brazil, but finally decided to interrupt my mathematical studies and spent 1945-46 in Rio de Janeiro (with a few days in Santos, São Paulo, Ouro Preto, and Belo Horizonte). Forty years later, I again met two of my fellow scholarship holders, at the 1985 Killam Prize award ceremonies; Pierre Dansereau also received a Killam Prize, and the distinguished artist, Jacques de Tonnancourt, attended the ceremonies.

It was in Brazil that I first began to collect books seriously. During my master's year at Toronto I had frequented the second-hand book shops in Toronto and had bought quite a few books; then, you could get, for a quarter, a book that will now cost you $25. Incidentally, a quarter was not to be sneezed at in 1945; as a graduate student, I had an annual income of only $600, and you could have a complete lunch for a quarter (or thirty cents if you splurged and did not have the cheapest thing on the menu). In Brazil, my scholarship was $100 per month, and I managed to spend at least half of that on books; when I returned to Canada, I mailed back 37 postal bags of books. Most of these were contemporary Brazilian works, but a few were from the nineteenth or even the eighteenth centuries.

Back in Toronto in 1946, I found myself wondrously affluent; I taught both Mathematics and Portuguese and thus had an income of $1600 a year, twice what most of the Mathematics graduate students had. I gravitated into the field of Algebra and did a Ph.D. in Finite Group Theory under Richard Brauer, one of the world's most distinguished group theorists. Richard had left Germany when Hitler rose to power, and Toronto was very fortunate in having him there for a number of years. He left Toronto in 1948 and went to the University of Michigan; 1948 was also the year when I finished my Ph. D. degree, and so it appeared natural to follow Richard to Michigan.

My first academic appointment was as a Lecturer at the University of Michigan, and I found the American educational scene very different from the Canadian one. In Canada, we had taken a very heavy undergraduate programme, but most of the Ph.D. requirement was for research. In the USA, one could enter the graduate school with very little background and, consequently, the Ph.D. programme had a very heavy course requirement in addition to the research thesis requirement. I was not unhappy at Michigan, but at that time I thought the University of Toronto was the most wonderful place in the world. So, when I was offered a position at Toronto in 1949, I came back to Canada and spent the next nine years on the Faculty of the University of Toronto.

5. Professional Career.

At Toronto, I taught both graduate and undergraduate courses, but the emphasis in those days was more on scholarship than on research. I had done my doctoral work in Group Theory, but Penrose had inspired me with an interest in Statistics. Also, Coxeter frequently had me referee papers in finite mathematics. I happened to mention to David Sprott that finite fields could be applied to statistical designs, and this led him to work in design theory for his Ph.D. under my direction; it also led me to become much more involved in Statistics and to teach graduate courses in Statistics to graduate students both in Statistics and in Engineering. My commitment to Statistics was also increased by the visit to Toronto of Sir Ronald Fisher. Fisher lived in Hart House, and we both had dinner in the Faculty Club there each evening; soon we got into the habit of sharing a table and I thus had the privilege of listening to the world's greatest statistician pontificate on a wide variety of topics over a three-month period. By 1957, when I left Toronto, I considered myself more involved in Statistics than in Group Theory; I was particularly interested in the Toronto Engineering Physics programme and had introduced a course in Statistics into the second year of that programme.

I had settled in at Toronto to a very comfortable existence and certainly had little thought of moving. Most of my research was classified, since I worked each summer, for several years, in the Communications Branch of the National Research Council; although this work did not lead to published papers, the habits of thinking that it developed were of much use to me later. Suddenly, a new horizon emerged; in 1957, I was approached by the administration of Waterloo College, a minor affiliate of the University of Western Ontario. I had once mentioned to Dan De Lury, who was then Head of Mathematics at Toronto, that I was interested in administration, and he recalled that remark when Waterloo asked him for advice on someone to head their Mathematics Department. Waterloo College was

Lutheran institution, and I probably would never have gone there if I had known as much about it as I learned later. Indeed, the main reason that I went was because several of my students from the Engineering Physics programme at Toronto seemed interested in beginning academic careers, and I was thus able to provide them with beginning jobs.

It turned out that the situation at Waterloo was much more chaotic than it had initially appeared. There were three major figures: C. L. Emery, who was promoting the idea of a co-operative Engineering school; President J. G. Hagey, who was set on the aggrandizement of a Lutheran Church College; and Dean L. H. Schaus who agreed with Hagey's aim but disliked Hagey very much. The story of the early politics at Waterloo does not belong here; Emery, whom Hagey disliked and feared, was quickly forced out and a battle arose between Hagey and Schaus for control. At one stage, there was even a Trinitarian-type dispute about the difference between the terms "Waterloo College Associate Faculties" and "Waterloo College *and* Associate Faculties". The net result was the formation of two Universities; Hagey was pushed out of Waterloo College, which became Waterloo Lutheran University. Hagey then became President of the new University of Waterloo, left the Lutheran Church, and became a strong proponent of the new institution.

While all this political manoeuvring was going on, academic life struggled ahead. In the early days, I can recall being one of the few academics with any experience in teaching an engineering curriculum; indeed, I ordered all the early engineering books for the library by simply buying all the texts that were in use at Toronto. But the situation was not encouraging and the success of the modern University of Waterloo is largely owed to one man. That man was A. R. Kaufman. At a crucial moment when our "academic advisory committee" had strongly recommended acquiring the land for the current large campus, and when other members of the board of governors either opposed or pooh-poohed the idea, A. R. spoke up in favour of the acquisition of the campus; such was his stature and influence that opposition immediately disappeared. The University of Waterloo was really born at that moment.

I was in Waterloo from 1957 to 1966 and served both as Head of the Mathematics Department and Dean of Graduate Studies; naturally one of my main concerns was with recruitment. Many of our graduate students and junior faculty members were recruited from the Engineering Physics programme at Toronto, and many of them are still at Waterloo to-day. The biggest single step forward for the Waterloo Engineering Faculty came when Doug Wright (currently President of the University of Waterloo) was induced to come from Queen's to become Dean of Engineering; this really started that Faculty on the road to excellence.

In the Mathematics Department, several appointments stand out in my mind. David Sprott, who had been my Ph.D. student at Toronto and who had done postdoctoral work in Britain with Penrose, came at the beginning. That was a time when applied statisticians were extremely scarce and David and I started raising our own brood; several members of the current Statistics Department began as students at Waterloo in those years. Later David was to become the first Dean of the Faculty of Mathematics.

Another important appointment was that of Kenneth Fryer; he had done his Ph.D. at Toronto, nominally under Brauer. However, since Brauer had left, I became his *de facto* supervisor, and we remained in close touch. Kenneth was a fine teacher and contributed much to our teaching capacities. Also, he and I set up a very active programme of visits to Ontario high schools in order to encourage mathematically gifted students to study at Waterloo. In order to have the students remember the visits, I normally wore a gaudy pink tie on such occasions; at a later time, the University of Waterloo Mathematics Society adopted such a pink tie as the emblem of the Mathematics Faculty.

I am most proud of the fact that we were able to induce Bill Tutte to leave Toronto and come to Waterloo. Tutte is one of the great mathematicians of this century, and has been the major developer of both graph theory and matroid theory. I always felt that his outstanding talents were not sufficiently appreciated at Toronto, and I was very happy that we were able to get him as a member of our faculty at Waterloo. Bill has since been named to the Royal Society of London, and received the Killam Prize in 1982; his is an outstandingly creative mind.

In the administration of Mathematics, three events now seem to be the major ones of my years at Waterloo. The first was creation of the Junior Mathematics Contest; this has burgeoned into what is now a whole series of mathematics contests that are written right across Canada. These contests have spurred many students on to do advanced study in Mathematics, and they have served as a model for the even larger Australian Mathematics Contest that Peter O'Halloran so ably runs from Canberra. The second was the introduction of a Co-operative Programme in Mathematics; by now, most of the Waterloo students in Mathematics and Computer Science follow this pattern whereby they alternate terms in academic study and practical employment. The third was the formation of the Faculty of Mathematics at Waterloo; this has given much greater visibility to Mathematics and Computer Science, has allowed them to compete with the Natural Sciences on an equal basis for funds and students, and has avoided the second-echelon status to which Mathematics and Computer Science are relegated at many universities.

My own personal development at Waterloo followed along two paths. On the humanistic side, I met Maurice Ettinghausen of Rosenthal Books in Oxford, who was undoubtedly the leading international expert in Portuguese bibliography. It was through him that I was able to acquire many rare and interesting volumes for my private Portuguese library (this was back when books were still "reasonable"; the big surge in book prices came in the seventies). Dr Ettinghausen had helped King Manoel of Portugal (who abdicated in 1910, and died 1935) to build up a great collection of Portuguese literature, and he had an unrivalled number of contacts in the bookselling and aristocratic world; when I knew him, he was ninety, but he was still as sharp as a whistle and possessed an immense fount of knowledge (he claimed to be the only bookseller in existence who had ever remaindered an incunabulum). I owe a great debt to Buchanan and Ettinghausen for giving me many years of satisfaction in book collecting, and I thought of both of them when I finally donated my collection to the rare book library at the University of Toronto in 1988 (the University has graciously termed it the "Stanton Collection of Portuguese Literature", but I must acknowledge that my expertise in the area of bibliography was built on that of Ettinghausen and of Buchanan).

On the mathematical side of things, my interests took two further turns. When I went to Waterloo, I realized that Computer Science would be of supreme importance in the coming years, and I tried to build up that area. Then, as now, there was a shortage of people in Computer Science; so I wrote an elementary text on Numerical Analysis and taught our first courses in that area myself. Then I was fortunate in being able to lure one of my former students at Toronto, Wesley Graham, away from IBM; Wes became the mainstay of early Computer Science development at Waterloo and is now Dean of Communications there. Later massive developments in Computer Science at Waterloo are undoubtedly due to the fact that we started working in the area early and that we had someone with Wesley's abilities at the helm.

I had moved from Group Theory to Statistics because of the interests of David Sprott. Now I had another change of direction, largely due to the work of my Ph.D. student Jim Kalbfleisch. Jim was a Statistics student who developed a keen interest in things combinatorial, especially in Ramsey Theory. He wrote a fine thesis in that area, and I was inevitably drawn in the direction of combinatorics. Later, Jim was to return to Statistics, and he became Head of the Statistics Department at Waterloo and, currently, Dean of the Mathematics Faculty. As things turned out, I have remained in the field of Combinatorics.

6. Time for a Change.

In 1966, with the formation of the Mathematics Faculty at Waterloo, I took a sabbatical year at the University of Manitoba. I have always found the "building-up stage" to be the most interesting, and Waterloo was now well established. It was time for me to move elsewhere, and David Sprott became the first Dean of the Faculty of Mathematics, a post that he filled with great ability. Bill Forbes, who was second Dean of the Faculty of Mathematics, liked to quip that Waterloo replaced me with two deans, two associate deans, and five department heads; I admit to being a workaholic, but I think that the replacement is less indicative of my habits than of the tendency for University bureaucracies to keep inflating themselves.

Up until 1966, I had done a reasonable amount of research, but my output could certainly have been greater. In moving from Group Theory to Statistics, then to Numerical Analysis, and then to Combinatorics, I had been a bit of a dilettante; if I had stuck with one area, I should probably have been able to achieve more. But I have no regrets at spreading myself around; I have built up a wide circle of mathematical friends in all these areas, and it is a humbling and rewarding experience to be able to associate with leading figures in several areas. Indeed, I often think that some contemporary mathematicians wear very narrow blinkers. When I run into certain mathematicians (some of them extremely gifted in their own areas), I am frequently disappointed at the parochial view they take of mathematics; it is a mistake to adopt a very puristic and theoretical view and assume that one's own area is immensely superior to all others (especially to those areas that might be tainted by the possibility of applications).

In my sabbatical year at Manitoba, I began serious work in Combinatorics, especially on problems related to Room Squares and on covering problems. I also began an enduring collaboration with Ron Mullin, formerly a student and a colleague at Waterloo. Perhaps this is the place to say a few words in answer to a question that was put to me at the University of Toronto Library just last week: "Can you explain to a librarian just what Combinatorics is?" My answer was that Combinatorics deals with problems of the following type: Suppose that you have a staff of 20 male and 23 female librarians; in how many different ways can you select a committee of seven persons so that the committee contains exactly three men and four women?

Of course, Combinatorics goes on from questions of such a nature to deal with more exotic ones, but that problem gives the flavour of the subject. Advanced design problems arise that have important appplications in agricultural experimentation, in coding of information, in transmission of data from satellites, in cryptography and computer security. But all the problems encountered depend on the basic fact that one is dealing with a finite set of objects.

My year at Manitoba left me firmly (and finally) in the combinatorial field. But I still needed a position. I was offered one at Manitoba, but I decided rather to go to York as Director of the Graduate Programme in Mathematics. However, I maintained a connection with Manitoba, through 1967-68, by flying out once a week to give a graduate course in Statistical Design.

I spent three years at York, but I now regard this period as an interlude. At that time, York was at a strange stage. They did not teach First Year Mathematics; instead first year Mathematics was given as one option under the heading "Modes of Reasoning". There were other unusual courses. One sophomore course was entitled "Eros in Western Literature"; this would have made a fine graduate topic in Comparative Literature but seemed strange when given to a group of callow undergraduates who did not even know their own literature, much less any foreign one.

In 1970, I made what was to be my final academic move (at least, so far). I went to Manitoba as Head of the Statistics Department; however, after two months, I was asked to take over Computer Science, since that Department had recently lost almost its whole personnel to industry and was in need of building up. I could not resist such an opportunity and have served in Computer Science since that time. In one way, this has been a return to an earlier interest; in another way, it represents a new venture. Increasingly, finite mathematics, especially combinatorics, has become the language of Computer Science. Indeed, the current chairman of the Society for Industrial and Applied Mathematics, who is much more applied than I have ever been, has stated: "Theoretical computer science has a distinct overlap with discrete mathematics in both problems of interest and tools. Discrete optimization and combinatorics are respectable occupations for either a mathematician or a computer scientist".

7. International Contacts.

Like most research workers, I have served on the editorial boards of various journals. That experience has given me a feeling for the need for rapid publication of results. A result needs to appear in print with the least possible amount of delay between submission and publication.

Such a remark may appear obvious, but I have encountered publication delays as great as four years. Because of this, we started up a new journal at Waterloo, with Janos Aczel as editor; that was *Aequationes Mathematicae*. The experience of setting up a journal was to be useful to me at Manitoba when we decided to inaugurate *Utilitas Mathematica* in 1972. We were fortunate to be able to get Frank Yates, who is probably the world's senior statistician, to serve as honorary editor, and we had a wide spectrum of interests represented on the editorial board: Derrick Lehmer in Number Theory, R. C. Bose and S. S. Shrikhande in Combinatorics, Jim Wilkinson in Numerical Analysis, David Finney in Statistics, Hanno Rund in Differential Geometry, to name just a few of our outstanding editors. Over the years, the journal has progressed from being set up on a Selectric typewriter to being computer typeset using custom software; but it has always maintained its aim of publishing articles with an applicable flavour in a wide spectrum of research areas. One of the high points of the existence of *Utilitas* came in 1982 when we published a special two-volume set in honour of the eightieth birthday of Frank Yates; the volumes were presented to Dr Yates at the British Experimental Farm in Rothamsted at a special luncheon attended by many of the elite of the statistical world, and he gave a most witty and interesting address reminiscing over statistical events of the last half century.

The year 1972 was an important year for me in one other way, since it began my "Australian Connection". I had met Jennifer Seberry at a conference in Calgary in 1969, and I knew that she and Walter Wallis were deeply involved in the nascent Australian development of Combinatorics; however, in 1972 they spent a term with us at Manitoba and we were later visited by Anne Street, who was likewise moving into the combinatorial field. My contacts with Anne, Jennie, and

Wal led to an invitation for me to give an address in 1974 to the Combinatorial Mathematics Society of Australia (which was not yet officially formed) and to the Australian Mathematical Society. From that initial visit to Australia was to result much collaboration; in 1976, we started the journal *Ars Combinatoria* as a joint Australian-Canadian Journal of Combinatorics, and it has now become one of the leading world journals in the area (Ron Mullin served as the first editor-in-chief). I have since visited Australia many times; indeed, I spent an entire sabbatical year there in 1977-78 working at the Commonwealth Scientific and Industrial Research Organization in the Statistical Division, which was headed by Joe Gani. The combinatorial collaboration between Canada and Australia has continued to grow stronger, and many students have gone from one country to the other; there has also been a very fruitful interchange of research workers at conferences and on visits. I have done as much as I have been able to do in encouraging this collaboration, and the University of Newcastle was kind enough to recognize my work with the award of an honorary D.Sc. degree in 1979; I have just learned that I am to receive a similar honour from the University of Queensland in 1989. These honours are, of course, very pleasant, but the important thing is not the recognition of an individual but the increase in international collaboration and research achievements.

Somehow, I seem to have become involved in other journals as well; back in 1970, Ron Mullin, David Roselle, and I, started the First Southeastern Conference on Combinatorics, Graph Theory, and Computing. There were 30 people at that first conference in Baton Rouge, Louisiana. In 1989 there were some 500 people at the Twentieth Conference in Boca Raton, Florida; the Southeastern Conference on Combinatorics, Graph Theory, and Computing has now become recognized as one of the major world conferences in the field, and the *Congressus Numerantium* series that includes the conference proceedings is an important record of research.

8. Thoughts on Research, the Past, and the Future.

Students often ask me how one gets involved in research; they claim that they can not think of a research problem to work on. This is probably true; I can remember having had the same problem many years ago. The fact is that one never does research by deciding "Now I am going to write a research paper". If you are working in any area, you naturally encounter problems of interest. Usually, you can not do these problems; so you may alter the problems until they become amenable to attack. There are always new avenues to explore; indeed, research activity breeds more and more problems. Everything you do leads to something that is related, and you end up with more interesting problems than you have time to work on. Indeed, I tend to think of mathematical research as the analogue of poetry; the result may be pedestrian but is frequently beautiful, and the creation of a mathematical theorem, or of an elegant demonstration of a known result, gives you the same sense of satisfaction and of wonder as when you create a particularly felicitous phrase or read a beautifully rhythmic line of poetry.

I have always thought that the time to involve students in research is when they are undergraduates, and I have had many rewarding experiences from taking junior students to research conferences. I always tell them: "Try to do something as soon as you understand the problem; the worst that can happen is that someone has done it before you. But, if you wait till you learn everything about any subject, you will always want to read something else and you will never get down to trying to attack something on your own".

The modern university has certainly changed a great deal over the past forty years. When I was an undergraduate student at the University of Western Ontario, we had Sherwood Fox as President, and the university could be truly described as a community of scholars; you could drop in and talk with Dr. Fox, or with Dean Neville, at any time. Even at Toronto, which was much larger, President Sidney Smith stated categorically that "The only reason for the existence of a central administration is to make life easier for the faculty; if the administration does not achieve this purpose, then it has no justification for its existence".

President Smith certainly believed these words and acted on them. I am afraid that very few modern administrators subscribe to his beliefs. Modern administrators seem to have a much smaller outlook, and seem to regard the university as just another form of business, with themselves as "important managers" and with the faculty as existing only to provide further importance for the managers. University senates tend to be filled with would-be politicians who value committee work above scholarship and who become tools of the administration in compliantly creating an ever-increasing morass of red tape through which the faculty must wade.

Indeed, I am not sure that the universities, as we know them to-day, will survive for another hundred years. I am reminded of the dissolution of the monasteries after they had moved further and further from their original aims. The monks were sure that there would always be monasteries; yet suddenly the monasteries disappeared and their functions of scholarship, investigation, and learning were taken over by the new universities. I do not know what will succeed our universities, but I think there is a real possibility that they will become so mired down in minutiae that are unrelated to their research and teaching missions that our descendants will see a "dissolution of the universities". In that case, the universities will be succeeded by some form of institution that revives their original goals.

Of course, one other change in universities has been the great increase in student numbers (or rather in the number of bodies enrolled; I doubt if the number of real students has increased). A contemporary American politician has even said that he looks forward to the day when 100% of young people will attend university. If his wish should be fulfilled, then we certainly will need a replacement for the universities. Aready, the universities have assumed too great a baby-sitting role; they are not challenging the capable students sufficiently. And they contain too many students who can not read and write competently, who look for a social life without work, and who can not really be called either literate or numerate.

And yet, despite these gloomy remarks, I am an incurable optimist. I have always adopted a Panglossian approach to life, and I really believe that, whatever you choose to do, things will always work out well in the long run. I see many bright spots in the current educational scene. Despite the silly criticisms of "rote learning" by some professional educators, there is an increasing awareness among students that some things simply have to be learned by rote (who ever became a great violinist without a great deal of rote practice?) There is an encouraging revival of interest in Latin and the "hard humanities". The top students are still doing splendid work even though university administrations have lowered their sights and are aiming at "increasing the retention rate". Indeed, the students who are faring worst in our current educational crisis are the vast body in the middle range who are neither extremely good nor extremely bad; they are not receiving enough challenges to make them reach their full potential, and some of them are even being encouraged to follow the paths of least work.

9. Finale.

Now that I have reached an age when I must accept the label of "senior citizen" (obnoxious term), I can still regard myself as fortunate. I am at a university that has given me the rank of Distinguished Professor; furthermore, my university does not force you to retire because of age if you are competent to continue. And I am still active and still get plenty of inspiration and stimulus from working with my research students.

Looking back over the past, I count myself lucky to have known many great mathematicians. Some are now a part of history, like Jim Wilkinson who was one of the most brilliant pioneers of numerical computation of the age. Others still active, like Bernhard Neumann at 80, are an inspiration to all of us and to students who are still entering the research scene.

I am also lucky to have had many exceptional students. While one must challenge all students and get them to work to the extent of their capacities, the real rewards come from cherishing those few students who are endowed with creative ideas; you can not take too much care in fostering their abilities and in promoting their development. You must always set them tasks a bit beyond their abilities and then try to get them to stretch their abilities to meet the challenge. By now I have published over 200 research papers, and a great many of them are the results of collaboration with students; we have both benefitted from working together, from merging our differing experience and our differing ideas.

My own challenge is to follow in the footsteps of that great chemist Joel Hildebrand who was still producing research students at age 98 when many of his earliest Ph.D. students had retired at 70. I may not get to that age, but it will not be because of lack of effort. I still recall the attitude of Uncle James, in the hay loft at

92, and I think that his insistence on remaining active is an attitude that we should all adopt. One of my favourite poems has always been Tennyson's "Ulysses", especially the lines

> How dull it is to pause, to make an end,
> To rust unburnished, not to shine in use.
> As though to breathe were life!

As long as there are brilliant students coming along in the next generation, there will always be a survival of learning and of research. Those of us who are already well advanced in our careers have the responsibility of nurturing and encouraging our successors. We may not see the future except darkly, but we know that somehow the radiance of achievement will be there.

RAYMOND N. YONG

(April 10, 1929–)

Killam Laureate, 1985

Present (1988) Occupation—McGill University

Yong is currently William Scott Professor of Civil Engineering and Applied Mechanics, Director of the Geotechnical Research Centre, Director of the Diploma Program in Waste Management—Groundwater Contamination, and Adjunct Professor of Civil Engineering in the University of Florida, U.S.A.

Birth Place/Family Details

Yong was born in Singapore, April 10, 1929. Raised in Singapore and Malaysia, he left Singapore for the United States upon completion of his high school education, in 1947.

His father (now retired) was a school teacher and Principal (Methodist Missionary English School); he taught Mathematics and Latin. His mother remained (as always) a very supportive mother and housewife.

Yong has one brother (older by two years) and one sister (younger by five years). They both completed their University education in Singapore. His brother studied medicine and obtained his MBBS, and subsequently his FRCS (London), FRCS (Edinburgh), and his M.D. degree. He has served as Professor of Surgery in the two major Universities in Singapore and Malaysia. His sister obtained her M.A. degree in English and Mathematics, and is Principal of the Methodist Girls' School in Singapore.

Yong's wife (Florence) was a school teacher, who is currently occupied with work concerned with horticultural activities. The Yongs have two sons; the older (Raymond T. M.) works as computer/program analyst, and the younger (Christopher) is completing his university training in Mechanical Engineering.

Early University Education—U.S.A.

Yong left Singapore in June, 1947, for the United States under the initial sponsorship of his godfather, Reverend Raymond D. Roche. Rev. Roche, who was a Methodist missionary sent from the United States to Singapore in the late 1920's, had returned to the U.S.A. and was administering a parish in Washington County, Pennsylvania.

Yong lived with Rev. Roche and his family while attending Washington and Jefferson College (W & J College) in Washington, Pennsylvania. Initially enrolled as a pre-medical student, he changed after the first semester to a major in Mathematics and a minor in Physics. His first degree was a B.A. (Maths. -Physics) from Washington and Jefferson College.

The high-valued principles held and practised by Rev. Roche's family had a significant influence on the development of Yong's attitudes towards education and service. These would become very important in shaping the later part of Yong's university education and research career.

Yong continued his university education, upon completion of studies at Washington and Jefferson College, at the Massachusetts Institute of Technology (M.I.T.) in Cambridge, Massachusetts. He completed studies in Civil Engineering at M.I.T. in 1952, obtaining his B.Sc. C.E. degree. He continued postgraduate studies at Purdue University in Lafayette, Indiana, concentrating on developing a better understanding of the nature of engineering materials. He completed studies at Purdue University, where he obtained his M.Sc. C.E. degree in 1954.

University Education and Canada

Between 1954 and 1956, Yong worked in industry in the U.S.A. while continuing further postgraduate study. During this period of "industry" work, it became clear that his previous academic training raised many questions concerning the application of the basic sciences to "engineering practice". The need to seek the many reasons for "adaptation of basic principles" for application to "standard practice", combined with a persistent curiosity to determine "why things behave the way they do", convinced Yong that he needed to continue his education.

The choice to continue studies at McGill University, Montreal, was made because it provided the opportunity to work as a Lecturer in the University while pursuing postgraduate studies and research. Working in a relatively new area of study concerning arctic soils and permafrost, and seeking advice at times from staff members from the Physics and Chemistry departments, he carried on research and formal studies to the M. Eng. and Ph. D. degrees in 1958 and 1960, respectively.

The completion of formal studies, however, was to serve only as the begining of a very interesting and rewarding career in research at McGill University. Because the system of postgraduate studies and research at the time when Yong joined McGill was less structured (in comparison to the present), it was possible to develop research into the many problems which one encountered whilst trying to implement the practice of Civil Engineering. In particular, the everpresent interest in the basic nature of engineering materials, and in the reasons why and how the nature of the materials controlled properties and influenced behaviour, provided the drive towards the studies undertaken with the research students and research team.

Because of innate curiosity and because of the basic training obtained at W. & J. College in Mathematics and Physics and at M.I.T., it became clear that there was a need to develop a more thorough understanding of the fundamental interactions that contribute to the formation and development of the various materials that constitute "engineering materials". To place this interest in the proper context, it should be noted that, at that time, it was not thought necessary to enquire into the basic structure and nature of materials. Standard practice required proper testing of materials to determine their physical and mechanical properties. Since the use of engineering materials generally required that they respond to applied sets of forces or circumstances, it was important that the proper response properties of the materials be obtained or developed through design requirements.

Why Science

The study of the physico-chemical properties of natural soils was motivated by the fact that (1) they are complicated materials and are not materials that are manufactured or produced as "design materials", (2) they are materials that are encountered as "everyday" materials in Civil Engineering practice, and (3) the use of soils as a support base for all constructed facilities, and their longterm performance, requires one to understand material behaviour and response performance characteristics in the context of a physico-chemical environment. The basic challenge was to obtain a good understanding of how the materials are formed in nature and how the various sets of regional and environmental controls exercised influence on the behaviour and response characteristics of the materials.

In essence, the principles learned in the study of materials science were applied to the study of soils—with greater emphasis on the interacting relationships developed between the molecular structures (and types of forces) of the constituents forming the soil particles and the chemistry of the fluid phase contained in soils. This formed the basis for many research studies on the physico-chemical behaviour of soils. It should be noted that the early reaction of the practising profession and of other academic colleagues working in the same engineering field was not very receptive to the kinds of work being reported in the literature as the outcome of the basic research. As it turns out, they were soon to learn that concerns over longterm stability of the materials, environmental impact, and land disposal waste management required the kinds of research undertaken and reported by Yong and his research group.

Unexpected Twists of Fate

The opportunity to come to the United States to study through the invitation and support of his godfather, Rev. Raymond D. Roche. Without this, he would have stayed in Singapore for his University education.

The decision to stay at McGill University after completion of the Ph. D. degree, even though the opportunity to continue research work (in the applied field) at M.I.T. was made available. The decision to continue into basic research in soils and snow was made easier because of the arrival of a new colleague in soil science, Dr. B. P. Warkentin, who was to play a major role in the continued development of Yong's research program. As it turns out, they were to write two books together which established the fundamentals of physico-chemical properties of soils, with particular attention to the behaviour and response performance of soils.

The decision to stay at McGill led to the opportunity to meet his future wife. This was perhaps the most important and significant twist of fate.

Most Exciting/Frustrating Research Experience

Most exciting experience — Gaining an ability to relate the basic sets of interacting mechanisms and forces from the various soil constituents to total soil performance and behaviour. Acceptance of the two basic textbooks, written with Dr. Warkentin as co-author, from the information and learning gained from this research work has been most satisfying.

Much of the new information learnt can now be put to use in addressing the major problem facing the population. This is the problem of control and management (disposal) of hazardous/toxic and other kinds of wastes. Ultimate disposal is on land! This includes disposal of wastes which have been incinerated—bearing in mind that the ashes, which generally contain higher concentrations of whatever toxic components cannot be incinerated, need to be disposed in a safe and secure environment. The use of the substrate soil/rock material as a disposal medium requires a good basic understanding of the nature of the subsurface soil/rock materials and their chemical, physical, and biological reactions to waste contaminants. The most exciting and rewarding research is the development of the necessary pieces of information (theories and models) that permit the materials to act as proper buffer material and thus avoid pollution of the groundwater, and hopefully permit environmentally safe disposal/containment of wastes.

Yong created and assumed the Directorship of the Geotechnical Research Centre at McGill University (1975); this is a multi-disciplinary research team working to provide basic and applied research for solution of industry problems in the field of earth and earth-associated environmental sciences. The nature of the research studies reflects the same concern for a proper understanding of the fundamental elements of interaction between the various constituents defining the earth-related problem. As an example, contaminant transport processes in earth substrate materials constitute a group of studies which have considerable impact on how wastes are managed and disposed.

Yong developed a new postgraduate Diploma program directed towards the study of Waste Generation and Control for protection of the environment. The study program is a direct outcome of the perceived needs of the profession as evidenced by the need for the kinds of research work conducted by the Geotechnical Research Centre in response to the critical waste management/disposal problems faced by municipalities and industry. The new graduate diploma program is meant to provide training for engineers, environmentalists and other concerned scientists, and legislators, in the field of study concerned with waste contamination of groundwater and the technology required to assess, evaluate, monitor, remedy, and design against such problems.

Most frustrating experience — Inability to obtain an adequate measure of support from industry and government. The most frustrating research experience is not that the kinds of experiments and studies are painstakingly tedious and time-consuming, but that the industries that are responsible for generating the wastes, that is, creating the problem, are not sensitive to their responsibilities. Government agencies and policy makers are equally insensitive to the magnitude of the scientific and technical problems. Without cooperative support from both these groups, it has been difficult to mount the intense and varied research needed if we are to solve the problem of environmentally safe management of wastes. The lack of proper equipment, physical support, and cooperation from industry, are some of the items that contribute to a frustrating research exercise! We know what needs to be studied, and also the various problems that need to be addressed. However, the means to pursue the needed research are, sad to say, not readily available.

Hobbies, passions, causes

Hobbies — Music (piano), photography, and sports (squash and tennis).

Passions/causes — (1) Safe environmental management of toxic/hazardous and other kinds of wastes, and (2) the need to return "something" back to the general region where one came from.

Item (1) is being addressed via present research studies and work with industry and government. Item (2) has Yong involved in research and development projects with various countries in South-East Asia (Thailand, the Philippines, Indonesia, India, Malaysia) aimed at increasing the quality of life by providing for better means and technology to manage surface and groundwater supply.

International contacts/activities

International activities have concentrated on work related to problems of waste management, water and groundwater quality, and land reclamation in developing countries, where Yong has worked with the local experts and local government officials and ministries.

Other international activities include active participation with various colleagues in other countries in development of standards, criteria, and awareness concerning the critical problem of "being buried by one's own waste"! In addition, Yong has lectured in various countries.

Professional experience and honours

Other than engaging in University research and teaching, Yong has always worked with industry in using basic research to solve industrial problems. As a result of this kind of cooperative work with industry, Yong has obtained more than 60 patents.

Yong has been involved with industry projects concerned with tar sands, crude oil delivery via pipeline from Prudhoe Bay, resource development, land disposal of hazardous waste, and high level nuclear waste disposal/isolation.

He is a Fellow of the Royal Society of Canada (F.R.S.C.), a Fellow of the Engineering Institute of Canada (F.E.I.C.), Chevalier de l'Ordre National du Québec, and a member of the various professional engineering societies in Canada, the U.S.A., and Great Britain. He has published three books and over 300 papers; also he has edited nine other books.

Hopes and concerns for future of sciences in Canada

It is acknowledged (and certainly well known) that funding support for research in the basic sciences from federal and provincial granting agencies is disgracefully inadequate. Much has been said about this, and about how much this affects present attitudes of research scientists, and the future scientists of this country. However, not much has been said about applied research and development, particularly in regard to the need to develop the technological edge for Canada. The cuts in funding support for NSERC have denied young applied researchers the opportunity to explore new technological ground in research, since this does not provide a sure source of funding. The stated government plan for industry dollar matching input to NSERC may be good and attractive in principle, but is severely constrained because of the lack of incentives for Industry and also because of the attitudes of Industry.

The need to provide for a better cooperative R. & D. program between university and industry research groups is obvious. However, this requires internship programs provided by Industry for academic staff members to educate the "professors" in the problems of real world application of academic research. Unless the incentives are found to provide for this kind of synergism, progress in applied research and technological development will be painstakingly slow.

It is not immediately clear how the long term picture will look, with the new policy of government research laboratories being required to implement full cost recovery research. One cannot help but feel that there is only a finite sum of research money available in the "marketplace" and that, if care is not exercised, this money will support more research centres than are necessary or indeed capable of producing proper and good research. There is a stark difference between research looking for good research centres (to do the research) and research centres looking for research to do!

Much is said about the environment, especially by public officials with less than adequate knowledge of what is being said. It is clear that, if the various responsible parties are not totally sensitized to the proper technical details of the problems of environmental pollution, we face the real prospect of eliminating the quality of life we now possess because of the inability or unwillingness of the responsible parties to learn! Scientists and engineers working with various parts of the problems cannot function to their full potential without full physical and monetary support from these parties. The significant problem of waste generation and requirements for proper environmentally acceptable management/disposal of waste is the pressing problem of our time. If we, as proper scientists/engineers do not solve the problem, it is not clear what the future generations will face, or what the future generations will say about our attention to our responsibilities during our watch.

Engineers and the future of Canada

If we are to become technologically competitive with the major countries, it is clear that more and better trained engineers and technologists are needed. Without adequate support from industry and government, we can not take the lead in technological development for those kinds of industry which provide the major service to Canada. We can only hope to follow the initiatives of others. Development of Canada requires engineers trained to be sensitive to the many factors and conditions that make Canada different from other countries. Adaptation of technology from other countries will not suffice.

We are one of the most wasteful societies in the world, and we are in danger of producing waste that will bury us all. Unless we begin to learn how to control our generation of waste, and unless we develop better ways of handling and disposing of this waste, we will not be able to find safe disposal havens that will insulate us and future generations from the many health hazards accruing from them.

JACQUES GENEST

(May 29, 1919-)

Killam Laureate, 1986

Jacques Genest was born in Montreal, Quebec, from a family with deep roots in that province. The first Genest came to Canada in 1666 as a lieutenant in the "Régiment de Carignan" and settled on the Ile d'Orléans where the Manoir Mauvide-Genest still stands as a historical monument. Jacques Genest's father was Professor of "Procédure civile" for over thirty-five years at the University of Montreal and a widely respected consulting lawyer. His mother came from the town of Arthabaska in the Eastern Townships, as did Sir Wilfred Laurier, Suzor Côté, and Armand Lavergne, among others. His maternal grandfather was the Parliamentary Representative of the county at the Quebec Parliament for many years at the same time that Sir Wilfred Laurier was the federal deputy and Prime Minister of Canada.

In addition to his Christian education, three key events in his youth helped shape his personality and his objectives: a) When he was about 10 years old he had read in the French monthly "Science et Vie" an advertisement from a German manufacturer of educational toys. On his request, his father bought him two kits, one on electricity and the other one on chemistry. These kits were highly educational and gave him a deep and lasting taste for science and all things based on logic and fact. b) During his early college year, his father took him to visit New York. What impressed him most for the rest of his life was the Museum of Natural History where the words of Theodore Roosevelt on Youth are inscribed in marble in the entrance hall:

I want to see you game, boys, I want to see you
brave and manly, and I also want to see
you gentle and tender.
Be practical as well as generous in your ideals,
keep your eyes on the stars and keep your
feet on the ground.
Courage, hard work, self-masterly, and intelligent
effort are all essential to successful life
Character, in the long run, is the decisive factor
in the life of an individual
and of nations alike.

c) During his internship year he was made Editor-in-Chief of the University of Montreal students' newspaper "Le Quartier-Latin". The motto of the journal became a key principle: "Bien faire et laisser braire" which can be translated literally: "Do well and let bray. Do well, that is, as long as one sincerely and honestly believes in what one sees as the truth, then go ahead and fight for it and let the others bray!"

He completed his secondary studies at Collège Jean de Brébeuf, where he obtained the B.A. degree "magna cum laude". He has always maintained a great and deep gratitude for his educators, the Jesuits, who gave their lives and all their time to the education of the young, even if he did disagree with several aspects of their education system because of its rigidity.

Already, during his college studies, Dr. Genest had decided to enter medical school because medicine, of all professions, best allied the scientific method and logic to compassion and love for his fellow man. During his youth, he was equally interested in chemistry, as he was in organizations for helping the poor, such as the St. Vincent de Paul Society. During his medical studies in the summers of 1938 and 1939, he took courses at Harvard University Medical School, first in surgical anatomy and then in physiology, under the great Walter B. Cannon. He received his M.D. degree from the University of Montreal, after a one-year internship, in 1942. He did three years residency at the University of Montreal Hôtel-Dieu Hospital, one year of which was spent in pathology as the first resident under Professor Pierre Masson who trained him in the scientific aspects of medicine under the traditional and so rewarding clinico-pathological discipline. He was profoundly influenced by Professor Masson who was one of the great pathologists of his time and truly one of the only representatives of scientific medicine in French-speaking Canada at that time.

Postgraduate Studies in the U.S.A.

Dr. Genest did postgraduate studies at Johns Hopkins Hospital, where he specialized in renal physiology, spending three years with Dr. John Luetscher and Dr. Elliot Newman. It was at Hopkins that Dr. Genest became acquainted with scientific medicine. One of his first papers was published in collaboration with Dr. Alfred Blalock when he studied the renal function of patients with coarctation of the aorta before and after surgery. Since he had decided to spend a good three years at Hopkins in renal physiology and the regulation of sodium, he thought he might as well obtain a Ph. D. degree. His Chief, Dr. Elliot Newman, referred him to the Dean, Dr. Allan Chesney. When told about the object of his visit, the old Dean said to Dr. Genest: "If you are interested in a postgraduate degree, then go to University X where you can obtain them fast and cheap. Here at Hopkins, what counts is the originality and quality of the work and of publications!" That was enough. Dr. Genest received a lesson which was key in his future scientific career. He stayed at Hopkins and never regretted it, although later in Montreal he advised his medical research fellows to "go for a Ph. D." if they were ready to devote three to five years to laboratory research.

He has never forgotten the weeks spent trying to measure plasma creatinine using an optical colorimeter in which the yellow color of the unknown had to be compared with a series of standards, a procedure which resulted easily in blurred vision from trying to match identical degrees of yellowness! In 1946, he spent a full summer at the Wilmer Department of Ophthalmology of Johns Hopkins Hospital to study the fundi oculi under the famous professor Allan Woods in order to achieve greater clinical competence in this crucial aspect of the physical examination of patients with renal and hypertensive diseases.

After taking a summer course in Organic Chemistry at the Harvard School of Chemistry, Dr. Genest joined the Rockefeller Institute in September, 1948, because of his interest in the regulation of sodium in hypertension and in the adrenal glands, which were known to be the major factor controlling sodium excretion. At Rockefeller, he learned many things which were to be very helpful in setting up the Clinical Research Institute of Montreal. From the great Dr. Donald Van Slyke, the father of biochemistry, he learned the basics in his first interview on arriving at the Institute. Dr. Van Slyke gave him five pipettes (1, 2, 5, 10, 20 mL) and told him to calibrate them with water and mercury and that these would be his pipettes for all of his work. It took a full month. How precious those pipettes were! He added that, if Dr. Genest did not have any specific research problem in mind, to come back to him later. (In fact, if a research fellow did not have a personal research problem within a few months, Dr. Van Slyke would lose total interest.) From Dr. Reginald Archibald, who succeeded Dr. Van Slyke, he learned how to set up a new laboratory, an endeavor which was extremely helpful when he later organized the Clinical Research Department laboratories at the Hôtel-Dieu Hospital in Montreal, and he acquired a sense of precision. From Dr. Vincent Dole, he learned critical sense and the importance of breadth of knowledge. From Dr. Tom Rivers, who was the Director of the Rockefeller Institute Hospital, he understood the importance of a proper milieu and of the conditions necessary for the researcher to do his work under optimal conditions. This sojourn at Rockefeller was a turning point in his career and he established deep and lasting friendships with colleagues from all over the world. This is where he learned how important it was, in the words of Dr. Detlev Bronk who was a Director of the Rockefeller Institute, to: "Choose the right man, back him up, and stay out of the way".

The atmosphere at Rockefeller was a very congenial one since almost all the research fellows were not married and were living in rooms overlooking the East River, with excellent meals and poor pay ($2,400 per year), but in an unequalled atmosphere of scientific exchange, constant pursuit of excellence and of the disciplines of research in terms of intellectual integrity, objectivity, clarity of thinking, and precision of expression. Research fellows were frequently found working late into the night until 2 and 3 in the morning, and working on Saturdays and Sundays. The only breaks in this routine were the "command performances" which came from Dr. Thomas Rivers, the Director of the Rockefeller Hospital, who needed partners for his game of tennis!

From his courses taken at Harvard and his long sojourns at Johns Hopkins Hospital and at the Rockefeller Institute, Dr. Genest realized how pontifical, dogmatic, and deficient the teaching in Montreal had been in physiology, biochemistry, and in clinical medicine. The great clinicians were those who made diagnoses on sight and who behaved as pontiffs! But there were some exceptions. The teaching of Surgery with Armand Paré and Donald Hingston was of great excellence, and so was the superb nursing care for which devotion to the patient and compassion were all important and for which the Hôtel-Dieu of Montreal was famous.

In 1948, he published in the Union Médicale, the oldest medical journal in Canada, an article entitled "L'avenir de la médecine canadienne française" which has been considered a landmark in the history of medicine in French-speaking Canada. In this article, Dr. Genest stressed the deficiencies in clinical teaching and practice, the absence of any full-time system, the absence of research laboratories, and deficiencies in the teaching of basic sciences. He proposed as key solutions the full-time system in university and teaching hospitals, the creation of research fellowships for young physicians interested in academic medicine, and stressed that the remarkable progress made in American medicine was due to the application of the experimental method. Finally, he emphasized the necessity for young French Canadian doctors to obtain their training where medicine and medical research were most highly developed, that is, in the United States.

The teaching of medicine and the scientific management of patients were aspects which needed reform and Dr. Genest was profoundly influenced by the way medicine was organized in America in the early 1940's, especially by great physicians like George Thorn who played a key part in his going to Johns Hopkins, by Warfield Longcope who was physician-in-chief at Hopkins, and by a pleiade of great clinicians and clinical investigators at that institution and of great researchers at the Rockefeller Institute.

In 1951, the Government of Quebec asked him to make an in-depth survey of the medical research and diagnostic centers in the U.S. and in Europe. This one-year survey allowed Dr. Genest to meet most of the key leaders in contemporary medicine, both in leading teaching institutions and in medical research institutes throughout the western world.

Three major ambitions became clear to Dr. Genest:

a) to become a top clinical specialist in his chosen field—hypertensive diseases;
b) to contribute by his research work to the body of knowledge concerning the basic mechanisms of hypertension;
c) to contribute to the progress of society by the promotion of the importance of clinical research for a better treatment of patients and prevention of diseases, by creating an environment for innovative and creative research.

His travels throughout the major teaching medical institutions in the United States and in Western Europe and his many discussions with the leaders of medicine led him to realize that clinical research was poorly organized, that laboratories were distributed haphazardly wherever there was space available in different wings and floors of university hospitals, that there was little communication and exchange between clinical scientists, and that there was a plethora of expensive equipment utilized only a fraction of the time. It became clear to Dr. Genest that it was time to provide a new concept for the more efficient organization of modern clinical research by concentrating all the "professional" (that is, full-time) clinical scientists in one geographical area with extensive auxiliary facilities at their disposal, the most sophisticated scientific equipment available, and with an administration *at the service of* research and researchers in a specialized clinical research institute. This concept progressively evolved in the mind of Jacques Genest and found its realization in the Clinical Research Institute of Montreal.

Return to Montreal

When Dr. Genest came back from his inquiry in Europe and in the United States in 1952, there was almost no medical research in teaching hospitals, except at the Royal Victoria Hospital in Montreal. In the francophone medical schools, research was done only by a few individuals, as a part-time occupation, under poor material conditions and without financial resources. Clinical research did not exist in the teaching hospitals. Sister Allard, one of the greatest women in the history of nursing, was then the Director of the University of Montreal Hôtel-Dieu Hospital. She was one of the few who realized the necessity of clinical research in a teaching hospital, and offered Dr. Genest the opportunity to set up a Clinical Research Department at that hospital. This was a challenge that fitted Dr. Genest's ambition and he accepted it. Despite strong opposition and severe difficulties in the first years, the concept of full-time hospital-paid clinical scientists with access to both research laboratories and patients became accepted, and in 1960 Dr. Genest was elected unanimously as President of the Medical Board of the Hôtel-Dieu Hospital. In 1966, he was made Chairman of the University Department of Medicine. The concept of full-time clinical scientists and full-time clinicians was established in francophone Canada as of 1960. In August, 1966, the University of Montreal Faculty of Medicine was granted a full five-year accreditation by the Association of American Medical Colleges. The comments concerning the Department of Medicine included the following statement: "A vigorous, well known, and highly respected individual of obvious competence has been made Chairman of the Department. He is rejuvenating the Department. Its climate is now that of 'open doors' and discussion". This was Jacques Genest.

One of the goals to which Dr. Genest devoted much of his energy was to open the field of research and of scientific medicine to French-speaking Canada. He insisted on the acquisition of competence, emphasized the importance of the pursuit of excellence in everything, of intellectual integrity, clarity and precision of expression. Only by achieving competence at the highest level could French Canadians come to be respected and achieve success. To this he added "opening of the windows" on the world and started regular exchanges with American scientists and institutions and established strong links with scientists in Great Britain, in France, in Germany, in Switzerland, in Sweden, and in South America. Gone were the days of pontifical and dogmatic statements, gone were the days of the routine acceptation of mediocrity. All this was not done without obstacles, jealousies, and fierce opposition, but Dr. Genest pursued his dreams without deviation and without stopping.

Scientific Contributions

Throughout his research career, the constant theme in his projects and clinical activities has been the study of hypertensive diseases in humans and the role of sodium which was believed to be a key factor in the pathogenesis of essential hypertension. His first scientific papers were with the late Elliot Newman at Johns Hopkins on renal function and the use of ammonium thiosulfate and of para-acetylaminohippuric acid as measures of glomerular filtration rate and of renal plasma flow, respectively, in normal subjects under conditions of exercise and in patients with congestive heart failure.

But it is his work under Drs. Donald Van Slyke and Reginald Archibald at Rockefeller Institute that persuaded him of the crucial importance of accurate, sensitive, and specific methods for the progress of our knowledge of biological phenomena.

Immediately after accepting from the University of Montreal Hôtel-Dieu Hospital the offer of setting up a Clinical Research Department, Dr. Genest directed all his energy along two major lines:

a) Organization of a Hypertension Clinic which has been in continuous operation since 1952, with two weekly clinics, and where antihypertensive drugs were studied at phases 1 and 2 and where a variety of patients with various types of hypertension were investigated and followed for long periods, some for more than 35 years. This Clinic became one of the largest in the world, with patients referred from Canada and many other countries and with an average of 9,000 to 10,000 patient visits per year in the last 20 years.

b) Expansion of his research projects initiated at Rockefeller Institute on the mechanisms of human hypertension and the regulation of sodium. In 1972, his multidisciplinary research group on hypertension was formally recognized by the Medical Research Council which awarded it one of its first large "group grants". This grant has been renewed every five years since and has been responsible for the stability and continued high productivity of the group.

From 1952 up to the present, Dr. Genest studied the clinical pharmacology of newer antihypertensive drugs as they came out from the various pharmaceutical houses. This gave him and the members of his group a great breadth of knowledge of the management of hypertensive patients while witnessing the great improvements in the treatment of hypertension in the the last three decades. He and the members of his group gave innumerable lectures at medical meetings on one aspect or another of the management of hypertension in humans. Drugs which were studied were Rauwolfia extracts, reserpine, rescinnamine, reserpidine, hexamethonium, hydralazine, ansolysen, guanethidine, bretylium, spironolactone, chlorthalidone, veratrum extracts, thyazide diuretics, alpha- and beta-blockers and converting enzyme inhibitors. His group was among the first to demonstrate that the lupus syndrome encountered with hydralazine was closely associated with doses higher that 200 mg/day and that the mental depression seen quite frequently in patients receiving reserpine was due to dosages higher than 0.5 mg/day and occurred especially if there was a past history of depressive episodes.

In 1956, he published a key paper in *Science* showing that human arterial hypertension was associated with significantly increased urinary aldosterone excretion. This was widely confirmed. His second major contribution was that of demonstrating that angiotensin II was the major factor controlling aldosterone, the key hormone regulating sodium. In this way the relationship between salt, the kidney (through its renin-angiotensin system), and the adrenal glands (through aldosterone) was established. New methods for the determination of urinary aldosterone, for plasma angiotensin II and renin activity were established, and these were used widely until the advent of the radioimmunoassay methods.

In 1966, he was the first to emphasize the importance of measurements of plasma renin activity under conditions of maximal stimulation (low sodium diet plus administration of a thiazide diuretic) for the diagnosis of true renovascular hypertension. In 1964, he demonstrated that the most important criteria for prediction of success of surgery in renovascular hypertension were:

a) a significant pressor gradient,
b) an excessive plasma renin activity in renal venous blood under stimulation and the absence of minimal presence of renal arteriolar changes in the involved kidney.

These new methods and their application were the major reasons for the organization of an international symposium on Angiotensin, Sodium, and Hypertension which was held in Ste. Adèle-en-Haut in October, 1963; the Proceedings were published as a separate volume of the *Canadian Medical Association Journal*.

His group was also among the first to demonstate the presence of "renin-like" enzymes in tissues other than the kidney, especially in the brain and arteries. Since then, other workers, among them a former student of Dr. Genest, Dr. Detlev Ganten, have demonstrated the existence of an independent renin-angiotensin system in the brain and in the heart, and others have obtained similar findings for arterial tissue.

In 1981, while chairing a Commission of the Government of Ontario for the evaluation of the scientific productivity of the Ontario Heart Foundation, Dr. Genest came across a research progress report from Dr. Adolpho de Bold of Queen's University in Kingston, who reported the rapid, marked, and short-lived effects of homogenates of rat atria on diuresis, natriuresis, and vasodilatation when injected into normal rats. Dr. Genest was very excited by this finding, since it might be the answer for the search for a natriuretic hormone, the isolation of which had baffled scientists for the previous thirty years. As soon as he returned to Montreal, this finding was quickly confirmed within a few days and a major effort was set up for the isolation, identification, and the study of the biological effects of the substance present in those rat atrial extracts. He enlisted the collaboration of all the members of the Multidisciplinary Research Group on Hypertension at the Institute and that of several other groups in the Institute, whose help was crucial for different aspects of this research program. The project was placed under the direction of Dr. Marc Cantin who had previously worked for over a decade on these atrial granular myocytes.

The Hypertension Group at the Clinical Research Institute was amongst the first to isolate the factor responsible for this biological activity. Arrangements were made with Merck & Co. in Rahway to synthesize the compound rapidly and a patent was issued by Ottawa in August, 1983. Within a few weeks, the compound had been synthesized and this was the first and only source available to ANF (atrial natriuretic factor) studies for several months, from August, 1983, to January, 1984. Within the next three to four years, major contributions were made by the Multidisciplinary Research Group and by several collaborators, including Drs. Nabil Seidah and Michel Chrétien, Dr. Peter Schiller, Drs. Jacques Drouin, Mona Nemer, and Ross Milne.

The variety and the fundamental nature of the many contributions made at the Clinical Research Institute of Montreal was a unique testimony to the value of the concept of the organization of modern clinical research as exemplified by the Institute. Not only was the active factor in these atrial homogenates isolated and sequenced, but more powerful analogs were synthesized, its gene was cloned and localized on the chromosomal map, a structure-activity relationship was conducted to find the active sites of the hormone, cardiovascular and pharmacological studies were done, the cellular messenger of its biological effect through guanylate cyclase was identified, its R-I receptor was isolated and identified. Studies were done of its effects on vasopressin, the antidiuretic hormone, and on aldosterone; its localization in tissues by autoradiography and immunocytochemistry especially in the brain, kidneys, lungs, liver and sympathetic ganglia; its effect in rats with various types of experimental hypertension, in normal subjects and in patients with essential hypertension. All these varied and highly specialized studies were done in-house, that is, within the Institute. No other institution in the history of medicine has ever made such a variety of fundamental studies on the same hormone within so short a time by a collaborative effort. This was the reason for the prestigious Research Achievement Award given to Marc Cantin by the American Heart Association in November, 1988, to recognize the major contribution by the Hypertension Group at the Clinical Research Institute, and also that of Dr. Philip Needleman in St. Louis, Missouri.

Preoccupied by the important role that emotional and psychosomatic factors play in aggravating hypertension just as it was noted by others for peptic ulcer, asthma, angina pectoris, colitis, skin diseases, and others, and considering the actual drug abuses, Dr. Genest published a paper on "The acquisition of serenity" as a substitute for tranquillizers. This was presented as the Convocation Address on receiving an honorary degree from the University of Toronto in 1970. In this paper, Dr. Genest stressed the importance and described some ways of having a more positive and serene attitude towards the problems of life as a way to relieve tension and anxiety and without the use of tranquillizers. One of his grateful patients even printed 10,000 copies for distribution to his employees and friends.

From 1952 to 1967, the year of the inauguration of the Clinical Research Institute of Montreal, Dr. Genest trained over 58 research fellows coming from various countries, several of them obtaining a M.Sc. or a doctorate degree from McGill University through an arrangement with the late J.S.L. Browne, who was Chairman of the Department of Experimental Medicine. From 1967 to 1988, some 458 research fellows and postgraduate students received advanced training in various disciplines at the Institute, with 98 obtaining a Master's degree and 77 a Ph. D. degree.

The Clinical Research Institute of Montreal 1967–

This Institute, inaugurated in 1967 by the late Daniel Johnson, Premier of Quebec, was the prolongation of the Clinical Research Department at the University of Montreal Hôtel-Dieu Hospital. On October 8, 1975, on the occasion of the inauguration of the expansion of the Clinical Research Institute by the Right Honorable Pierre Elliot Trudeau, Prime Minister of Canada, Claude Ryan wrote in *Le Devoir*: "La science médicale québécoise vivait l'un des jours les plus encourageants qu'il lui ait été donné de vivre depuis longtemps ... Ce que l'on connaît moins, c'est l'ardeur exceptionnelle avec laquelle le Docteur Genest consacre depuis des années le plus clair de son action à la promotion d'une science médicale proprement québécoise en même temps qu'universelle par sa qualité et son rayonnement. Dès son retour au pays à la suite d'un long séjour qu'il fit naguère à l'Institut Rockefeller, le Docteur Genest s'était juré de doter le Québec d'un centre de recherche médicale de premier plan. Sacrifiant à cette cause les postes et les chances d'avancement que lui offraient à profusion les milieux anglo-canadiens et étrangers, il a tenu parole. L'événement d'hier était un hommage mérité à l'action persévérante et tenace qu'il mène depuis 20 ans pour l'avancement de la science médicale au pays."

As a result of his travel throughout America and western Europe, and of his many discussions with the contemporary leaders in clinical medicine and biomedical research, Dr. Genest elaborated the concept of the modern organization of clinical research. His travels in the period of 1940–1950 had shown the lack of organization of clinical research, that laboratories in university teaching hospitals were established in a haphazard way depending on the space available on various floors and wings, with little exchange and collaboration between the various clinical scientists and their research fellows. Hence came the concept of clinical research institutes entirely autonomous, but closely affiliated with university hospitals, or of clinical research laboratories integrated in the geographical area of a university hospital. The purpose was to concentrate in one area all the "professionals" (*i. e.* full-time) of clinical research and their laboratories in one given area for interaction, collaboration and exchange. Such institutes or specialized clinical research areas included all the auxiliary facilities necessary: a specialized research library, animal facilities, meeting rooms, cold rooms, a common instrumentation room where expensive equipment not utilized at least 75% of the time by one researcher could be shared by all, common glassware washing facilities, specialized cell culture rooms, a separate administration under the scientific director and *at the service of* the researchers, monthly statements of the monies spent under each grant obtained by a researcher, a separate auditorium, machine and electronic shops, computer and statistical services, and others. Such institutes should be integrated or closely affiliated with a medical school for the granting of postgraduate degrees (M.Sc. and Ph. D.) and of academic titles (professor, associate professor, and assistant professor). Special attention was given to maintaining an atmosphere conducive to the pursuit of excellence, work well done, and harmony among the researchers. The value of such a concept has now been demonstrated by the many institutes organized on the model of the Clinical Research Institute of Montreal, not only throughout Canada but throughout the world.

No better example of the success of this concept can be given than that of the major and diverse contributions on the atrial natriuretic factor which was studied in so many of its biological aspects through the *in-house* collaboration of various research laboratories in the Institute. At no time in the history of endocrinology and that of any research institution has so much knowledge been contributed in the course of so few years by so many scientists on so many aspects of ANF biology and chemistry.

Right from the beginning of the Institute, Dr. Genest insisted that his research fellows should acquire laboratory training equal to that of their Ph. D. colleagues, or obtain, in addition to their M.D., their Ph. D. degree. In this way, clinical scientists could acquire training as clinical specialists equal to that of their clinical colleagues and laboratory training equal to that of the Ph. D.s and could in this way become leaders in bringing unity in medicine and in research instead of jealousy and conflicts. At IRCM, M.D.s and Ph. D.s were placed on the same salary scale according to years of training, with special awards for major contributions recognized nationally and internationally. Each clinical scientist and Ph. D. was allowed two half-days a week for consulting either with patients, with industry or teaching institutions, in order to provide more adequate incomes.

One of Dr. Genest's greatest sources of pride is to have attracted to the Clinical Research Institute top researchers and clinical scientists from many countries such as Professors Otto Kuchel, Vincent Beroniade, Pavel Hamet, Ernesto Schiffrin, Peter Schiller, Yves Marcel, Malur Sairam, Sri Pande, David Roy, and several others.

Teaching is optional for each researcher, except for the training of graduate students and research fellows. Some researchers do no formal teaching while others find it profitable to give a few lectures per year to medical students. There is only one standing committee, which meets in a statutory way every month to discuss administrative and scientific problems of general interest. Other committees are set up "ad hoc" and are disbanded immediately after their recommendations have been made to the Scientific Director.

All senior researchers receive their salary from the Institute through a budget awarded by the Quebec government, but they must obtain research grants from private or public bodies with referees and peer review systems to pay for technicians, animals, materials and supplies. Such an arrangement is already a sign of the quality of the research projects undertaken. A second evaluation is a two-day retreat taken yearly outside of Montreal at which the directors of laboratories present their work and their research projects for the future. At such retreats there are always two guest scientists from Europe, the United States, or Canada, who spend the whole two days with the researchers and make an individual report of their overall assessment of the quality of the work and the existing atmosphere to the Chairman of the Board of Trustees of the Institute. A third and key evaluation is that of site visits held every five years and lasting four days. These site visits are done by outstanding scientists such as Drs. Louis Siminovitch, Henry Friesen, Ed Rall, Sir Chris Booth, the late Claude Fortier, and Gordon Dixon.[1] These three ways of checking the quality of the scientific productivity of the Institute and its organization explain in good part the success achieved in the last two decades and its prestige throughout the scientific world.

If a researcher is unable to obtain financial support for his research projects after having been given a second chance, then his inability to support his own research projects and his technical personnel makes it clear that his talents would be better utilized in teaching, in medical practice, or in working for an industrial corporation. A small endowment fund which the Institute has built up throughout the years is reserved for emergencies, unforeseen circumstances, or the hiring of new researchers.

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[1] The Committee of the 4-day site visit in 1982, chaired by Dr. Louis Siminovitch and including Sir Chris Booth, Director of the Clinical Research Centre of the MRC of Great Britain, and Dr. Philippe Laudat, Director of the INSERM, states: "In summary, no matter which criteria were used, the site visit team was highly impressed by the quality of the research team of the Clinical Research Institute of Montreal. The Institute compares more than favorably with similar types of institutions anywhere in the world".

Bioethics Research Centre

Dr. Genest was among the first in North America to stress the importance of bioethics to the members of the medical profession and to the public. In early 1970, he foresaw the impact of the accelerating progress in biomedical research and molecular biology (recombinant DNA, gene transfer, …), especially of research dealing with the fundamental aspects of life and the future of our society. He therefore set up a Bioethics Research Centre within the Institute — inaugurated in September, 1976, with René Dubos as the keynote speaker — with members having privileges equal to those of the scientific researchers, in order to arouse the government and the public and to make them aware of the gravity of some of these problems in terms of eugenics, active euthanasia, gene manipulation, and others. This Bioethics Research Centre was the first one in Canada and in the world physically integrated within a biomedical research institute so that the bioethicists could be better acquainted with the status and the trends of biomedical research and on the motivation of the researchers. It took almost three years of inquiry throughout North America and Europe before finding the "right man" who was Dr. David Roy, an American teaching at Laurentian University in Sudbury, Canada, and who is a theologian, philosopher, and mathematician. Exactly one year after the establishment of the Bioethics Research Centre, the Royal College of Physicians and Surgeons of Canada asked Dr. Genest to give its Annual Lecture on the subject of "Bioethics and the leadership of the medical profession". This was given wide publicity. Since then, the Bioethics Research Centre has attained international recognition because of the importance of the field and because of the energy and competence of its Director. This was the birth of bioethics in Canada and a model used in several other countries.

When the Royal Bank of Canada awarded Dr. Genest its prestigious prize in 1980, a large part of it went to create a Fund to honour his parents, the annual income of which goes to the support of the Bioethics Research Centre.

Professional and Societal Contributions

Dr. Genest has, throughout the years, been a strong promoter of clinical research in Quebec and throughout Canada. In 1959, he founded the "Club de Recherches Cliniques du Québec", a club without formal organization and in which any clinical scientist interested in applied medical research could be a member. It has become the meeting place for all the francophone medical researchers throughout Quebec and its annual meetings are now attended by over 400 researchers and their postgraduate students. In January, 1962, he organized the first symposium in Canada on the subject of Clinical Research at the time of the meeting of the Canadian Society for Clinical Investigation.

Dr. Genest was also closely involved, as a founding member, in the Canadian Society for Clinical Investigation, of which he was twice President. He was also a founding member of the Quebec Heart Foundation. In 1963, he conceived the establishment by the Quebec government of the Quebec Medical Research Council which he served for six years as President. The Council has now become the "Fonds de la recherche en santé du Québec" with an annual budget of over 35 million dollars in 1988. This Council has been widely recognized as a major contribution to medical research in Quebec, especially for two of its most innovative programs. The first was the "subventions d'établissement", which consists of grants up to $25,000 to promising young researchers returning to Quebec after their periods of training in the United States or in Europe, in order to help them get started in their research work and enable the Province to "catch up" in biomedical research. The second program provides salaries for biomedical researchers to supplement the inadequate university capacity to employ young biomedical researchers. There are at present more than 118 "boursiers-chercheurs". These two programs, the first conceived by Dr. Genest, and the second one by Dr. Michel Chrétien who succeeded Dr. Genest as the Scientific Director of the Clinical Research Institute of Montreal, have been hailed as the "jewels" of the "Fonds de la recherche en santé du Québec".

Dr. Genest also served on several committees of the Medical Research Council for twenty-three years, from 1953 to 1976, of which eight were as Chairman of the Clinical Investigation Committee and eight as member of the Council. He also served for fourteen years on committees and on the Council of the Royal College of Physicians and Surgeons of Canada.

In 1959, he was asked to serve as a member of the Board of Trustees of the University of Montreal up to 1964, at which time he resigned to accept the post of Chairman of the Department of Medicine of the University. His many contributions to medical education, not only at the University of Montreal, but throughout Canada were recognized in 1988 by the Christie Award of the Association of the Professors of Medicine of Canada.

More than anyone else, Dr. Genest was the one who opened Quebec to scientific medicine based on objective observation and critical experimentation and to the importance of research, especially in the medical field.[2] By establishing the Clinical Research Institute of Montreal on the basis of a new concept, he provided the basis and the material organization for creating an optimal environment for innovative and creative scientists. Every week for the past 35 years, guest scientists and lecturers, mostly from the United States but also from all over the world, were invited to deliver lectures and seminars, in this way encouraging exchanges and collaboration with the best leaders in the world and enriching the "milieu scientifique" from the expertise and the friendship of those scientists. Long before the "révolution tranquille" in Quebec in the 1960 period, Dr. Genest had "opened the windows" on the world and played a leading role, with Professor J.S.L. Browne of McGill, in establishing close links between the "two solitudes" in the medical field in Quebec. He was convinced that the only way to success was not through negative endeavors and destructive criticisms, but only through positive attitudes and objective achievements.

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[2] In an article entitled: "The top 50 powers in the medical establishment", *The Gazette* (Nov. 26, 1977) wrote: "Dr. Jacques Genest dragged French Canadian medicine into modern times almost all by himself. Until he came along, it was rooted in wispy classical scholarship. He introduced research based on solid factual evidence. Founder in 1967 and still head of the Montreal Clinical Research Institute, he brought in and trained the best medical research brains in the province".

One of his major interests has been the study of the socioeconomic aspects of medicine. In April, 1959, he was one of the very first and few Canadian physicians to write publicly on the necessity for a National Hospital Insurance. This was before the Hall and the Castonguay Reports. He also made a thorough study of the National Health Service in Great Britain and of other national health systems in Sweden, in France, and in Switzerland. Since the establishment of the National Health Insurance in Quebec, he has written extensively on the deficiencies of the Canadian system because of bureaucratic intolerance of any advice from members of the medical profession and lack of expertise of any government technocrats in the complex domain of medical care.

Dr. Genest edited three textbooks on hypertension, one published by Springer-Verlag in 1972 and the two others published by McGraw-Hill in 1977 and in 1983. The latter textbook on "Hypertension: Physiopathology and Treatment" has been considered as *the* reference book on hypertension throughout the world and used by all those interested in the field. In addition, his name appears on 689 papers, on one-third of which he was senior author.

More Personal Notes

Dr. Genest has been the only Canadian physician invited to record his memoirs in the Series "Leaders in American Medicine", first for the Josiah Macy Jr. Foundation and the National Library of Medicine of Washington, and a second time for the Harvard Medical Library. In the first instance, he was interviewed by Vincent P. Dole, one of his mentors and great friends and one of the world's greatest scientists, at Rockefeller University; the second time, he was introduced by Dr. Michel Chrétien, with Professor George Thorn and William Cassels as discussants.

He has received 12 honorary degrees, one of the most prestigious being from the Rockefeller University on the occasion of the 75th anniversary of the Rockefeller Institute Hospital in 1986. The awards conferred upon him are many: Gairdner Award in 1963, Companion of the Order of Canada in 1967, Flavelle Award of the Royal Society in 1968, Stouffer Award of the Council for High Blood Pressure Research of the American Heart Association in 1969, the scientific Prize Marie-

Victorin of the Government of Quebec in 1977, the Royal Bank of Canada Award in 1980, the F.N.G. Starr Award of the Canadian Medical Association in 1982,[3] and the Killam Award in 1986.

He is one of the Canadian physicians highiy regarded by his American confreres in being invited to be a member of the "Society of Scholars" of the Johns Hopkins University, of The Association of American Physicians, The Peripatetic Club, and The American Clinical and Climatological Association of which he was Vice-President in 1987. He received several Distinguished Achievement Awards: from Modern Medicine in 1976, from the Canadian Society of Hypertension in 1985, from the American Heart Association in 1985, and from the Canadian Society of Endocrinology in 1986. Throughout his career, he has given many named and keynote lectures at various internal congresses and symposia throughout the world and was an invited guest scientist of the Hungarian Academy of Sciences in 1973, of the U.S.S.R. Academy of Sciences in 1968, of the Cardiac Society of Australia and New Zealand in 1980, and the Chinese Academy of Medical Sciences in 1983.

In order to be involved more closely and acquire a broader experience in societal affairs, he accepted a nomination to the Board of Directors of Merck & Co. , Inc. , of Rahway, New Jersey, which is the leading pharmaceutical company in the United States and in the world. Later, he also agreed to be a Director of the Montreal Trust Company which is part of Power Financial Corporation. After his retirement in 1984, the Royal Bank of Canada and the Clinical Research Institute jointly established in 1985 the "Jacques Genest/Royal Bank" chair in biomedical research in recognition of his many and diverse contributions.

In 1970, he was made "Sims Commonwealth Travelling Professor" of the Royal College of Surgeons of Great Britain and travelled to Africa and the British Isles, including Ireland, to deliver lectures and to meet the key scientists in what was the former British Commonwealth. In 1979, he received the high distinction of "Master" of the American College of Physicans. He is a certified specialist in internal medicine, endocrinology, and nephrology of the "Corporation professionnelle des médecins du Québec". For over fifteen years, he was one of the very few Canadian members of the Century Association and Club of New York.

Dr. Genest has been blessed in his marriage with a wonderful and great wife who was an inspiration and a constant support in his work. They have five children, two physicians married to lady physicians, and three daughters.

Outside of his professional obligations and his own research projects, his hobby has been reading, especially of history and biographies of great men, scholars, and scientists, and of the Roman and Greek classics, particularly of the Stoic School. Cicero and Seneca are two of his great heroes. Probably because of his many years in the best institutions in the United States and his great admiration for the contributions of America, not only in the political field of true democracy and freedoms, but also in the sciences, Dr. Genest has been a strong advocate of free trade and of a common market with the United States.

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[3] Citation: "Within a very short time, Dr. Genest had recruited an elite group of young researchers attracting and developing a team that now ranks among the most prestigious anywhere in Canada or abroad".

He has two major preoccupations for the future. One is the creation of scientific complexes or centres in cities like Montreal where various institutes attached to universities and devoted to fundamental research in the basic medical sciences, chemistry, physics, biology, plant pathology, and experimental psychology, would be grouped in the same area and integrated with a large university medical centre for rapid transfer of basic knowledge and technology to clinical medicine and the benefits of patients; the second is a proposal which Dr. Genest made in his keynote lecture at the Scientific Day of the University of Toronto on the occasion of its centennial in June, 1988. This proposal was the establishment of a one-year university program that could by called something like: "Perspectives and human values in the history of mankind". This proposal derives from the general consensus among educators and scholars in Europe and in North America, that one of the major deficiencies in our modern education system and in our contemporary society consists in the fact that young people are prepared to face life and the future as skilled technicians, as super-specialized graduates with little scholarship and without broadness of knowledge, and without a sense of human values throughout mankind's history. Each specialty has its own language, making communications difficult in addition to the evident lack of culture and liberal education in most spheres of society.

Dr. Genest believes that higher teaching institutions should give students a global and integrated knowledge of mankind, its history, its discoveries, and its destiny. The proposal put forward by Dr. Genest would be for a one-year program of broad education and overview of human knowledge towards the training of a "moderately educated person". It would be a modern version of the Arab ADAB and would involve courses on our knowledge of the universe, the history of mankind and its accomplishments in all spheres of human activities. It would integrate chronologically and horizontally all facets of human knowledge as it developed through the ages. Such a course would be somewhat different from the Great Books Program proposed 40 years ago by Adler and Hutchins and other similar programs in various universities.

Lectures would involve such subjects as:

a) history of mankind;
b) description and history of the development of the sciences (physics, mathematics, chemistry, astronomy, …), music, arts, medicine, law, major religions, social and political sciences;
c) the teaching of the great philosophers of the world (Socrates, Plato, Aristotle, Thomas Aquinas, …);
d) the ways proposed by great scholars to acquire wisdom and achieve a

happy life (Confucius, the writings of the Stoic School of Cicero, Seneca, and others).

This one-year academic program organized on a trans-faculty basis would have as its main objective to give young people and other applicants (from fields of communications, teaching, law, journalism, medicine, politics, …) a global and integrated knowledge of mankind and of its evolution, and to provide a better understanding of the world in which they live in relation to what has been done in the past, what is done elsewhere, and what should be done in the future in terms of human values, man's accomplishments, and destiny.

WILLIAM HOWARD RAPSON

(September 15, 1912 —)

Killam Laureate, 1986

William Howard Rapson was born in Toronto, Ontario, of English parents, Alfred Ernest Rapson and Lillian Jane Cannicott. Although they lived only ten miles apart in Cornwall, England, they met and married in Chicago, Illinois, U.S.A. In 1912 they moved to Toronto, when his father became an accountant with Canadian General Electric Company. Although his parents had only elementary school education, they had decided that their child would go to university. In his early life he did not know that it was possible not to go to university!

At age five, before he went to Morse Street Public School, Howard had decided he was going to be a medical doctor and that goal was maintained until grade thirteen in Riverdale Collegiate Institute. In grade ten he was fascinated by physical geography and became interested in chemistry. He started reading books on the subject, which was not taught until grade twelve. In grade eleven he bought the grade twelve chemistry textbook, read it, and did all the problems in it for the sheer enjoyment of chemistry.

At that time, all students had to write provincial exams in every subject. It was not necessary to go to school to matriculate; anyone only needed to pay two dollars each to write the provincial examinations in the required subjects. So in grade eleven Howard paid two dollars and wrote the grade twelve chemistry exam; he obtained first class honours, without taking any classes in the subject. Therefore, in grade twelve he took grade thirteen chemistry instruction and passed the provincial exam. In grade thirteen, in 1929, he decided that chemistry, not medicine, would be his future. Since the great depression had just begun after the big stock market crash, a friend told him it was easier to get a job as a chemical engineer than as a chemist. He decided that from high school he would go into chemical engineering.

No job.

When he graduated in chemical engineering in 1934, Howard tried hard, but he could not get a job. He had no intention to do research or to teach, but Professor Boswell, who taught organic chemistry, had liked Howard's fourth year research and got him a grant of three hundred dollars from the school of Engineering Research for the five months in the summer so that he could continue research. In the fall, his Professor got him a half-time demonstratorship for five hundred dollars per year so he could do research and become a graduate student. In January, 1935, a full-time demonstrator left to go into industry and Howard was promoted to demonstrator at one thousand dollars per year.

"Full time" then was thirty-eight and one-half hours per week of laboratory teaching, with time out for taking graduate courses. Research had to be done in evenings and on weekends.

In 1937 Howard Rapson married Mary Margaret Campbell. Married women were not allowed to teach school in those days and it was expected that they would stop working in offices. In order to survive, Howard had to teach industrial chemistry in a university extension course on Monday nights, organic chemistry in Danforth Technical School on Tuesday and Thursday nights, and tutor the Manager of Miles Laboratories, a mathematician, to convert him into a chemist, on Wednesday nights. The latter ultimately passed the examinations of the Canadian Institute of Chemistry!

Research.

Howard Rapson has always wanted to understand how things work: the earth; the solar system; the universe; human physiology; chemical reactions; electricity; machinery; everything. Perhaps that is why, along with teaching, he has spent his whole career in laboratory and industrial research.

In his fourth year in chemical engineering, for his first research project in the spring term of 1934, Rapson was assigned to work under the supervision of Professor Maitland C. Boswell, who taught organic chemistry. Dr. Boswell asked him to try to make inositol, hexahydroxycyclohexane. Boswell knew little about inositol. He confidentially told Rapson that he thought diamonds could be made somehow by removing water from each of the carbon atoms of hexahydroxycyclohexane. He swore him to secrecy, because he was afraid people would think he was crazy. He did not ask Rapson's opinion! Rapson was happy with the problem of trying to make the mysterious inositol.

It was not known at that time that meso-inositol, one of the nine stereoisomeric forms of hexahydroxycyclohexane is a component (bios 1) of the vitamin B complex. It is a dietary requirement for both lower and higher forms of animal life; hence it is one of the vitamins.

Rapson found that the only significant information about inositol was a long treatise on the subject by an English biochemist (Michael Polanyi, father of John Polanyi, Nobel Laureate), published in German in a German journal. He spent his whole Christmas holidays translating the Englishman's long paper into English.

There is a small amount of inositol in meat. Therefore, his first preparation of the compound was a small amount from meat by a tedious process. Then he read that inositol was found in larger concentration in the urine of students after consuming a lot of beer. Rapson at that time was not an alcohol drinker, but he thought it might have been the large consumption of water rather than the six percent alcohol which caused the elimination of inositol. He, therefore, consumed several litres of water (with great difficulty) and extracted inositol from his urine. As far as he is aware, Dr. Boswell did not pursue his concept of producing diamonds from inositol!

For his Master of Applied Science degree, Professor Boswell asked Rapson to work on the production of oxalic acid by alkali fusion of cellulose, a project investigated by some of his previous students. Along with sodium oxalate, some sodium acetate, sodium formate, and sodium carbonate are formed. Rapson found a catalyst and established conditions which would convert the sodium acetate into sodium oxalate and thereby greatly increase the yield of sodium oxalate from cellulose, from lignin, and from wood. The processes would have been commercialized, but just at that time a fermentation process was developed for the cheap production of oxalic acid.

Although he was a very kind man and a good organic chemist, Professor Boswell gave no guidance to Rapson during his Ph.D. research, in which he extended his work on sodium hydroxide fusion to pure compounds containing oxygen. Ultimately he could predict quantitatively the results from the fusion of any simple organic compound. He wrote his thesis after he went to Hawkesbury. From the secretary of the department, he learned that it was sitting on Dr. Boswell's desk for several weeks unread. Finally he wrote to Professor Watson Bain, Chairman of the Department (and first President of the Chemical Institute of Canada), saying that he would like to obtain his Ph.D. at the June, 1941, convocation. Bain took the thesis from Boswell's desk, read it, asked Professors Smith and McLaughlin to read it, then went to Professor Boswell and said: "We have read this thesis and think it is good. Will you please read it?" Dr. Boswell then read it and accepted it without any suggestions for improvement or correction. At the June convocation, Howard Rapson received his Ph.D. from Sir William Mulock, then Chancellor of the University of Toronto, Canada's first Postmaster General, who lived one hundred and two years.

The Hawkesbury years.

In 1940, the Rapson's daughter Margaret was born. Since his total annual income was only eighteen hundred dollars, which was not enough to maintain a family, Howard left the university to become a Research Chemist in the Pioneering Research Laboratory of Canadian International Paper Company in Hawkesbury, Ontario, at three thousand dollars per year. In 1941 he obtained his Ph.D. for his thesis on "Alkali Fusion of Organic Compounds". In 1942 he was appointed head of the Pioneering Research Laboratory. That year, their twin daughters, Linda Mary and Lorna Jean were born. In 1949, their son, William Howard, Jr., was born. They now have nine grandchildren.

In Hawkesbury, Rapson developed the combined cold and hot caustic soda treatments to produce very pure cellulose for manufacturing cellulose acetate and other purposes. He also showed, by systematic study of specially synthesized hydroxylated phenols, that branched-chain nonylphenol with ten condensed ethylene oxide units in a hydrophyllic side chain is the best surface active agent for removing resin from pulp used for making rayon. It still is, forty years later. Incidentally, it is now also a widely used spermicide. His main contribution in Hawkesbury was the development of chlorine dioxide bleaching and, with Morris Wayman, the invention of a commercial method of manufacturing it in pulp mills.

In October, 1952, Rapson was invited to re-join the staff of the University of Toronto as Professor of Chemical Engineering, at a much lower salary than he was earning in Hawkesbury. The President of CIP Research Limited told Rapson that he would have a substantial increase in salary and would succeed him as president, if he would stay with the company. While he was enjoying his life in Hawkesbury and his work in the research department, his wife Mary said Howard would not be completely happy until he got back into teaching and academic research which could be published. She decided in favour of accepting the invitation.

On January 1, 1953, Howard Rapson returned to the Department of Chemical Engineering at the University of Toronto as Professor, in the same position in the hierarchy of the department as he would have been in if he had never left. In 1976 he was honoured by being the first engineer to be appointed University Professor, for a five year term. Although he officially retired in 1978, in 1981 when his University Professorship terminated he was reappointed Professor of Chemical Engineering so he could continue research with graduate students. Rapson stopped laboratory research in April, 1988. To date he is author or co-author of one hundred and eighteen published papers and inventor or co-inventor of fifty-eight patents, some in forty-five countries.

Although he is best known in the Pulp and Paper Industry for his development of chlorine dioxide for bleaching pulp and of processes for manufacturing this explosive toxic gas on site in pulp mills, Rapson's research has involved fundamental studies of the mechanism of the reactions of chemicals with wood pulp and the reactions which form chlorine dioxide. The latter studies showed that the reaction of chlorate ions with chloride ions and hydrogen ions to form chlorine dioxide, chlorine, and water is fundamental in all processes for making chlorine dioxide. This led to the patented R2 process for making chlorine dioxide which became the most used process in North America by 1967. A solution of sodium chlorate and sodium chloride and strong sulphuric acid are continuously fed into a reactor through which air is bubbled and passed through two absorption towers in series. The chlorine dioxide formed dissolves in water in the first tower; the chlorine is absorbed in sodium hydroxide solution in the second tower; and the air passes to the atmosphere through a vacuum pump. The whole system is operated under vacuum to avoid having chlorine dioxide or chlorine escaping into the atmosphere. The resulting concentrated solution of sodium sulphate in sulphuric acid overflows from the generator. This solution is usually used as make-up chemical in kraft pulp mills, thus decreasing the cost, and making very cheap chlorine dioxide at a cost very near to that of equivalent chlorine. If a reducing agent, such as sulphur dioxide or methanol, is continuously added to the generator along with the sodium chlorate, salt, and sulphuric acid, the chlorine formed is reduced to chloride to be used over again to react with the chlorate and acid, thereby producing almost pure chlorine dioxide.

A related patent involves carrying out the same reactions in a combined generator, evaporator, and crystallizer, in which crystalline sodium sulphate is continuously removed from the generator. In this case, only chlorine dioxide and chlorine leave this generator to be absorbed in water. This process has become the one most used world-wide. If methanol is added to this generator to reduce the chlorine formed to chloride to use over again, twice as much chlorine dioxide can be produced per unit of time in the same generator.

In 1954, with a graduate student named Bogdana Brajsa, Rapson developed the CEDED sequence: Chlorination; caustic Extraction; chlorine Dioxide; caustic Extraction, chlorine Dioxide. It was presented in a meeting at the University of Florida and published. It has since become the most widely used pulp bleaching sequence, though there have been modifications of various kinds, such as adding peroxide or hypochlorite to the second extraction stage, introducing gaseous oxygen into the caustic extraction stage, or adding a hypochlorite stage to have a six-stage sequence.

Pulp is normally mixed with bleaching solutions and passed through a large tower with one to six hours retention time. The normal plant with five stages of bleaching retained the pulp for about twelve hours. In 1965 Rapson conceived the idea that pulp bleaching might be sped up substantially if the bleaching chemical solutions could be kept in continuous motion with respect to the pulp fibres. He tried this out in his laboratory by passing the chemical solutions successively down a glass tube filled with a pulp suspension. He found that five stages of bleaching could be carried out in nineteen minutes! He applied for a patent and published a paper on what he called "Dynamic Bleaching". At the end of the presentation of the paper, he challenged the equipment manufacturers to develop the equipment to carry out dynamic bleaching on a mill scale. In 1973, he received a telephone call from Knut Dahl, President of Kamyr A.B. in Sweden, to tell him that they had such equipment operating in Finland and invited Rapson to go to Finland to see it, which he promptly did. Today, there are seventeen such large plants, two of them in Canada, some producing as much as fifteen hundred tons per day of bleached kraft pulp in two vessels.

With Bertil Andersson in 1965, Rapson presented a paper on "Mixtures of Chlorine Dioxide and Chlorine in the Chlorination Stage of Pulp Bleaching" in which they showed that, in the first bleaching stage, replacement of a substantial part of the chlorine with chemically equivalent chlorine dioxide has many advantages, including nearly eliminating the formation of chlorinated organic compounds.

Modern methods of analysis have recently found extremely low concentrations of 2,3,7,8-tetrachlorodibenzodioxin (2,3,7,8-TCDD for short) in paper products. Although the concentration of this dioxin is so low that it will not harm anybody, public concern has caused pulp mills to use his findings and greatly increase the substitution of chlorine dioxide for chlorine in the bleaching process to solve the dioxin problem. The production of chlorine dioxide is rapidly increasing in pulp mills throughout the world. Chlorine dioxide does not produce dioxins, because it removes lignin by oxidation and not by chlorination. Furthermore, Rapson has shown that chlorine dioxide does not form mutagens on reaction with lignin and that the mutagenicity of chlorination stage effluent decreases linearly to zero as chlorine dioxide is substituted for equivalent chlorine in the first stage of pulp bleaching.

With Rapson as inventor or co-inventor, patents have been issued in the following forty-five countries:

> Argentina; Austria; Australia; Brazil; Belgium; Canada; Chile; Colombia; Congo Republic; Czechoslovakia; East Germany; Egypt; El Salvador; Finland; France; Guatemala; Holland; India; Italy; Japan; Liberia; Mexico; New Zealand; Norway; O.A.M.P.I.; Paraguay; Peru; Poland; Portugal; Rumania; South Africa; Spain; Sweden; Switzerland; Turkey; United Arab Republics; United Kingdom; United States; Union of Soviet Socialist Republic; Uruguay; Venezuela; Vietnam; West Germany; Yugoslavia; Zimbabwe.

Interesting Incidents.

A lot is said about people having goals and ambitions. With one or two exceptions, I have had almost no goals or ambitions in my life. Most events in my whole career have been happenstance, opportunities which arose by chance, which I recognized and exploited. From the time I was born, I did not know it was possible not to go to university.

When I was five years old, we lived at 137 Morse Street in the east end of Toronto. Morse Street public school was just up the street, near Queen Street. I used to play in the school yard. I became friendly with Mr. Markle, the principal, a kindly old gentleman. One day he said to me:

"What are you going to be when you grow up, Howard?"

Without hesitation, I replied: "A lawyer."

"Why?" he asked.

"So I can get my father's money back," I said.

My father had invested all his meagre life savings in a new venture, the True-Set Wheel Company. He was to be the treasurer. The wheel was a good idea, a tractor wheel with retractable lugs which could be extended for pulling a plough and retracted for travelling on roads. However, the president absconded with all the money people had invested in the company. My father lost all the family money, about two thousand dollars, and I was determined to get it back!

Mr. Markle said: "You shouldn't be a lawyer, Howard. Lawyers are a dime a dozen! You should be a doctor, not a lawyer."

I loved that old man so much that I immediately changed my goal in life. From then until grade thirteen, I was going to be a doctor! I worked hard in public school and in high school to reach that objective.

In 1926-27, in my second year in Riverdale Collegiate, a course in physiography (physical geography) started my interest in chemistry. I read books on chemistry from the public library and started a small chemical laboratory in the basement of our house at 6 North Place, which later became Edgewood Gardens! Chemistry was taught in fourth year high school. By Christmas of my third year, in 1927, I had learned so much chemistry that I bought the fourth year textbook, read it, and did all the problems in it. At Easter, 1928, I asked my physiography teacher, who also taught chemistry, if he would let me write the fourth year chemistry examination surreptitiously and mark it unofficially to see how well I could do on it. He agreed. I got over seventy-five percent, first class honours!

All students had to write province-wide examinations then. Nobody had to go to school to matriculate. By paying two dollars, anyone could write any examination. By passing the appropriate subjects, it was possible to obtain senior matriculation and go to university without going to high school. I paid the two dollars, wrote the fourth year chemistry examination and got first class honours in the subject, without having taken instruction in it. I felt great!

I still intended to become a doctor, but the consequences of having passed that chemistry exam were substantial. In the fall of 1928, when I entered fourth year (now grade twelve), I went to the principal, Mr. Thomas Kennedy and said I wanted to take fifth year chemistry.

"No!" he said, "You can take it next year with the rest of the class."

I said: "If you won't let me take upper school chemistry, I will do the same as I did last year. I will buy the textbook, pay two dollars, and write the provincial examination. You can't stop me from doing that!"

Mr. Kennedy said: "All right. If you feel that strongly about it, you can take chemistry with the fifth formers this year."

Thus, I took chemistry with fifth form students, who resented very much having this young upstart fourth former in their class. However, I was the only one in the class who got first class honours in the subject when we wrote the provincial examination in 1929.

Incidentally, the year before, when I won the oratorical contest, the fifth formers were upset that a third former had won this prestigious contest!

All students who wanted to were allowed to take fifth year English in fourth year; so I did that. The teacher was Captain Watson, who later became principal after Mr. Kennedy. As a result, I already had written and passed Ontario provincial examinations in three senior matriculation subjects when I entered fifth form: English Composition; English Literature; and Chemistry.

In those days boys and girls were in separate classes until after junior matriculation. As a result of taking chemistry with the fifth formers, I had to rearrange my whole timetable. I had to take Latin (which was compulsory then) with a class of all girls! In November, 1928, Miss Lewis called on me to translate some Virgil. I had not done my homework and bungled the whole thing. Miss Lewis scathingly attacked me:

"Rapson, I used to think you were a good student, but the way you are going you will fail Latin this year."

Imagine my embarrassment, just turned sixteen and being dressed down in front of a whole class of girls! I said nothing, but gritted my teeth and said to myself: "Miss Lewis, you will never catch me again!"

After that, I prepared my Latin assignments very thoroughly every evening. Miss Lewis did not call on me again until April, but then I stood up and rapidly rattled off a correct translation.

"Very good, Rapson!" she said. I glowed with pride!

This incident also had another effect. I had still intended to be a doctor until the beginning of the fifth year, when I thought: "Howard, why are you going into medicine when you are so interested in chemistry?" After a lot of thought, I abandoned my long-held goal. I decided to study chemistry at the university. At that time, a friend of mine in Rhodes Avenue United Church, Emmerson Skelton, was a student in chemical engineering at the University of Toronto. He told me that it was easier to get a job as a chemical engineer than as a chemist. So I decided to go into chemical engineering.

One day Miss Lewis stopped me in the hall.

"Rapson," she said, "You are not taking Latin this year?"

"No, Miss Lewis," I replied, "I am going to S.P.S. (the School of Practical Science, which had become the Faculty of Applied Science and Engineering). I don't need Latin to get in there. There are no scholarships for S.P.S.; so there is no point in taking Latin."

"But you were one of my best students! I wish you would take Latin."

Because I liked her so much, I said: "For you, Miss Lewis, I will take Latin!"

That is how Latin authors and Latin composition became my thirteenth subject in which I wrote provincial examinations in the twelfth and thirteenth grade. I have never regretted that decision. Latin has helped me in English, French, Spanish, even to some extent in Russian, when we travelled in many countries. I regret very much that Latin is no longer taught in most high schools. I am sorry that I only had time to take Greek for three months and did not complete the course. Another regret is that I did not have time to take Modern History, in which I was very interested. The subjects in which I passed provincial examinations were:

Physics; Chemistry; Botany; Zoology; Algebra; Geometry;
Trigonometry; English Composition; English Literature; French Composition;
French Literature; Latin Composition; Latin Literature.

Serendipity has played a substantial role in my life!

No job when I graduated.

The irony of fate was that although I went into chemical engineering because it was supposed to be easier to get a job, when I graduated I could not get a job.

That turned out to be one of the luckiest things that have happened to me! I tried all the petroleum refineries in Toronto, because I thought that was the industry a chemical engineer should be in, but nobody was hiring in 1934, in the middle of the Great Depression.

That is the fact which led to me becoming a researcher and eventually a professor. I had no intention to do research or to teach. Since I could not get employment, because he liked my fourth year research and wanted to pursue it further, Professor Boswell got me a three hundred dollar grant from the School of Engineering Research to continue my research for the four months in the summer. In the fall of 1934, since he still wanted me to continue my research, he got me an appointment as a half-time demonstrator, at five hundred dollars for the academic year. I registered as a graduate student for the M.A.Sc. degree. At Christmas, Bill de Montmorency left to join the Pulp and Paper Research Institute of Canada in Montreal. I was promoted to full-time demonstrator at one thousand dollars per year! My teaching career, combined with research, was launched, a career I had never even contemplated. Serendipity!

University.

The Jansen lunch club

Vogt Jansen, in 1935, was a demonstrator and a Ph.D. candidate under Professor J.W. Bain, working on the utilization of lignin, in a nice laboratory in the mining building. The few graduate students and demonstrators in the mid-thirties used to have lunch in Jansen's room along with Art Hunt, our glassblower, of whom everybody was very fond. A delegated person would buy bread, butter, ham, cheese, peanut butter, jam, etc., and we were allowed to put as much of these as we wanted onto the bread. The charge for lunch was twelve cents per person, except for Howard Rapson. He had to pay fifteen cents per lunch, because he ate so much more than the others.

We became very close friends. The discussions of our research, science, engineering, the department and the people in it, philosophy, the world, politics, etc., were very valuable.

Harry Sandford

Harry Sandford was one of the very early Ph.D. candidates in our department, perhaps number five or six (I was number nine). Under Dr. Boswell, he was working on the production of sulphur from sulphur dioxide and hydrogen sulphide, using a catalyst, which later became a very important industrial process widely practised today.

Sandford was also an ardent bridge player. In order to have time to play bridge, Sandford built an ingenious, very complicated glass apparatus to measure, circulate, and mix the gases involved, which would carry out his experiments over a long time (even overnight if needed). The results obtained were not spectacular, but it was rumoured that the committee had passed his thesis at oral examination because they were so impressed by the remarkably clever automatic equipment he had built (with the aid of Art Hunt) to carry out his experiments.

Bill Bowman

Bill Bowman was another demonstrator, who was doing research on thiophene under Professor Ardagh for his Ph.D. In 1939, I did not get a summer research grant; so we did not have enough money to live on and pay the rent of thirty-five dollars per month for our nice five-room duplex on Edgewood Avenue. We had to sublet our apartment to Bill Bowman and his wife and move up to the French river to a cabin owned by Fred and Ruth Western, friends of ours in the United Church Young Peoples' Union. We were able to live there free, requiring only enough money for our groceries. During the summer the Westerns came up to the cabin for two weeks, and we had to move out down the river and pitch a tent for that period.

This proved to be an interesting and important summer. We had been married for two and one-half years and had no child, although we wanted one. I attributed this to the fact that, in order to earn enough money to live, I was working very hard. I was teaching every day as a demonstrator. On Monday evenings I taught a course in university extension for twenty dollars per night. On Tuesday and Thursday evenings, I taught organic chemistry in Danforth Technical School for ten dollars per night. On Wednesday evenings, I tutored the manager of Miles Laboratories Limited for five dollars per hour. He was a mathematician who had married the owner's daughter and wanted to be converted into a chemist. I was able to teach him enough chemistry that he passed the Canadian Institute of Chemistry examinations and became a professional chemist. In addition to all this, I was treasurer of Rhodes Avenue United Church and superintendent of the intermediate Sunday School, and was active in the United Church Young People's Union. No wonder we were not producing children.

After that wonderful summer of complete relaxation, Mary became pregnant and Margaret was born on May 28, 1940.

Professor Ardagh

There was an interesting incident in Professor Ardagh's Industrial Chemistry Laboratory. Toronto Boy Scouts had been caddies at Bigwin Inn on the Lake of Bays for many years. I went there first in 1927, when I was fourteen years old, and was a caddy for five years. In the summer of 1932, when I had just finished second year in chemical engineering, I was hired as the cook at the Bigwin Inn caddy camp. I had to get up at five o'clock every morning to prepare breakfast for about fifty caddies. Some of the older boys and I became interested in certain waitresses. We went out with them every evening and never got to bed until midnight. Getting only five hours sleep per night and working very hard all day, I was very tired by the end of the summer. Ever since then, I have had great difficulty staying awake in lectures.

At that time, if any hour of the thirty-eight and one-half hour university week was not occupied by a lecture, all students had to work in the laboratory. We had no spare hours. In the fall term of 1932, on Tuesdays we had the morning hour from nine to ten without a lecture and therefore had to go to the laboratory for that period. One Tuesday morning Professor Ardagh was chatting with a group of students when I said:

"Professor Ardagh, since a single hour is not very useful in the lab, don't you think it would be better for us to sleep in an extra hour on Tuesday mornings and be more alert for the rest of the day?"

After a long pause, while he looked around the circle of students, he said: "Rapson is a fine one to talk. He sleeps through all my lectures!"

After another pause he continued: "But there is a funny thing about Rapson. He goes to sleep, but whenever I say something important, he wakes up and writes it down! Then he goes back to sleep!"

He didn't let us stop coming to the lab on Tuesday mornings. We were programmed for the whole thirty-eight and one-half hours per week, including Saturday mornings. Today not only has Saturday morning attendance been abolished, but by rule programmed hours for lectures and laboratories must be limited to thirty hours per week!

Explosions.

In 1932, in the spring, there was a very serious explosion on the fourth floor, in the fourth year research laboratory. A student doing his research under Professor Boswell had planned and carried out an experiment without Dr. Boswell's approval. He had put benzene and liquid oxygen into a sealed high pressure autoclave not equipped with a rupture disc or a pressure release valve and started heating it in an oil bath. As would be expected, ultimately the pressure became exceedingly high and the autoclave burst with a violent explosion which I and all others heard throughout the building. The student was standing over the vessel at the time and his subclavian artery was ruptured. After the students in the room recovered from shock, they jammed cloth into the bleeding wound. He was rushed to the nearby Toronto General Hospital and a lot of student volunteers gave him blood transfusions for three days, but he finally died.

This was a great tragedy in the Department of Chemical Engineering and Professor Boswell literally turned white-haired overnight. I had heard of such a phenomenon but had not believed it possible up to then. It had a devastating effect on Dr. Boswell and the rest of the staff. Ever since that event, no student is permitted to carry out an experiment without carefully studying all the safety aspects of the proposal and obtaining specific approval from his research supervisor. Although there have been many minor accidents, fortunately none has caused significant hospitalization or loss of life in any laboratory in any department in the Faculty of Applied Science and Engineering in the University of Toronto since that tragic accident.

My explosions

I have had two serious explosions and one serious fire in my laboratory experience, all resulting from lack of sufficient forethought. During my research as a graduate student, I used a glass thermostat consisting of a large bulb filled with toluene as a sensitive expansion chamber and a capillary tube containing mercury which rose to contact two wires to switch on a heater in a water bath. On repeated contacting, the mercury metal became corroded. To clean the thermostat I poured out the toluene and the mercury. Without drying it and without thinking of the consequences, I poured nitric acid into the tube to dissolve the mercury and heated it above the flame of a Bunsen burner on the top of the bench. After a few minutes the tube exploded, shattering the glass device right in front of my nose. Nitric acid splashed on my skin and drove particles of glass into my nose and cheek. Fortunately my glasses saved my eyes. I had forgotten that toluene and nitric acid would form TNT! Art Hunt was our glassblower and our first aid man. Whenever he heard an explosion, he grabbed a large wad of cotton batting and ran toward the source of the sound. He came into my lab, which was right next door, saw blood running from my nose and face. He grabbed my nose with the large wad of cotton batting and held onto it tightly all the way to the emergency room of the Toronto General Hospital, which was only about three blocks away. In the emergency, the doctors neutralized the nitric acid, picked out all the particles of glass they could find, stopped the bleeding, and sent me home. Although my skin turned yellow from the nitric acid, it eventually peeled and no scar was left. I was very lucky that I was not seriously injured or marked by that explosion, and especially lucky that nothing went into my eyes. However, there were strange consequences of that explosion. Several years later I would spit up particles of glass that had very slowly worked their way through the tissues from my nose and cheeks into my throat.

My second explosion occurred in the preparation room of the organic chemistry laboratory. In a four litre conical flask, I had dried ether with sodium metal, which reacts with water to form hydrogen and sodium hydroxide. I then poured the ether into a bottle, and placed the flask in a deep sink. Without thinking about the possible consequences, I ran water into the flask to destroy the remaining sodium. Immediately there was a loud and violent explosion! I had overlooked the fact that the sodium and water would produce hydrogen and a lot of heat and that the hydrogen and ether and air would explode.

Fortunately the deep sink absorbed most of the flying glass. However, I knew that I had been hit in the face and rushed out of the room, across the hall and down the stairs heading for the first aid room. Without seeing him, I flew past Art Hunt, with his hand full of cotton batting, rushing up the very wide stairs followed by a lot of students heading for the sound of the explosion. A very large piece of glass from the side of the flask had hit my face flat and stuck there. If that piece had hit my face edge first, it would probably have cut it very deeply or perhaps killed me. After the flat piece of glass was removed from my skin, I survived that explosion without a mark on me.

The fire

The other serious laboratory accident occurred in the Pioneering Research Laboratory of Canadian International Paper Company in Hawkesbury, Ontario. I was sitting at my desk when I heard a very loud scream from the adjacent room. I jumped up, ran to the door and saw Marion . . . in flames rushing out of the room through the opposite door. I ran after her yelling to her to stop, but she didn't! In the hallway, although I had never played rugby, I tackled her, knocked her to the floor and beat out the flames with my bare hands, for there was nothing else there to do it with. Just then a girl came around the corner, saw what I was doing (but didn't see the flames) and turned and ran back down the hallway shouting: "Dr. Rapson is beating Marion!" Soon a crowd gathered and we were rushed to the Smith Hospital and our burns were treated. Marion's legs were seriously burned and she was kept in the hospital. She recovered quite well. I don't know whether or not she had any permanent scars. Both my hands were burned, but they were bandaged up and I was sent home. That afternoon my parents came from Toronto to visit us. My mother was shocked and very upset when she saw my bandaged hands. However, I reassured her that the burns were not very serious and that my hands would recover fully and would not be scarred.

Safety lessons

The two explosions and the fire account for the fact that for the rest of my life I have been extremely conscious of laboratory safety. In fact, my first introductory lecture in the Organic Chemistry Laboratory, of which I was in charge for over twenty years, was always on safety, using my own experience to impress its importance on the students. Furthermore, I would put a question on safety on every written test, such as: "Where is the fire blanket located in the laboratory?", or "where is the water shower located?", or "what would you do if you got a droplet of caustic soda solution in your eye?", etc.

ClO_2 explosions

I blew up the first commercial chlorine dioxide plant in Canada at six p.m. on June 1, 1945, at the Temiskaming, Quebec, pulp mill of Canadian International Paper Company, which proved to be a very important event in the history of the development of chlorine dioxide bleaching. That day Morris Wayman and I started up the first chlorine dioxide plant in a pulp mill in North America, perhaps in the world. It was the Rapson-Wayman patented process, using two packed columns. Into the first one, concentrated sodium chlorate solution was introduced at the top to flow down over the ceramic packing. At the bottom, sulphur dioxide burner gas was introduced to flow up the column, reacting with the sodium chlorate solution to produce chlorine dioxide gas, which was carried with the nitrogen into the bottom of the second column. Water flowing down the second column absorbed the chlorine dioxide and the solution flowed out the bottom through a glass pipe to the bleach plant, to be used for pulp bleaching. This process was patented.

After starting up the plant in the morning with two operators in training, Mac McLeod and Bob Brown, we agreed at noon that Morris and I would work twelve-hour shifts, each with one of these men to do the testing. About six p.m. the production of chlorine dioxide was low. I assumed that the very small flow of concentrated sodium chlorate solution was not being distributed uniformly over the small ceramic Raschig ring packing in the column because of the fast gas flow. Therefore, I stopped the vacuum pump, stopping the air flow for exactly one minute and then pushed the start button. Immediately there was an explosion, which filled the atmosphere with toxic chlorine dioxide. I realized immediately what had caused the explosion. I had forgotten that the solution would continue to produce chlorine dioxide in the tower but there would be no air flowing through to dilute it. The concentration of chlorine dioxide in the solution would increase. When the air flow began again, the concentration of the released gas would be above the spontaneous explosive limit. So it exploded immediately. The air flow must never be stopped without stopping the flow of sodium chlorate solution and stripping the chlorine dioxide gas out of the solution remaining in the tower with air. I donned my gas mask, stopped all flows, and went down stairs to examine the damage. The first tower had split, but had not come apart. The glass piping feeding the sodium chlorate solution had broken. I knew that, if we did not get the plant running that night, it would be the end of the whole chlorine dioxide development. I saw that the plant could be repaired. I called Morris to come in with Bob Brown. I called Harry Sparling, the maintenance man on that shift, to have some steel bands made and bring rubber sheeting to line them to pull the cracked sections of the tower together. Since the plant was operated under a slight vacuum, I asked him to bring putty to fill up the cracks and bailing wire to support the feed system at the top. By midnight we had the repairs completed and started up the plant. It ran perfectly. At four a.m. we shut down the plant and all four of us went home for a few hours of sleep.

At nine a.m. I went to see the mill manager, Herlag Anvik, and told him that our new chlorine dioxide plant blew up last night. His face showed horror and that meant that he would not allow this new fangled chlorine dioxide process to be used in his mill, which would be the death knell of the whole chlorine dioxide development. However, I said we knew exactly what caused it, we knew how to prevent it, and it would never happen again. We had repaired the plant by midnight and it operated successfully for four hours. We shut it down and went to bed. We had established a new process, a world first!

This proved to be a very important fortuitous event. As a result we were able to invent better and more efficient processes for making chlorine dioxide safely. Now, in 1990, more than a million tons per year of explosive, toxic chlorine dioxide are used world wide. Although generators occasionally explode because of improper operation, they are designed to accept these explosions. Very few people have been injured, but no person has been killed by the explosion of a chlorine dioxide generator. Today there are single vessel generators producing sixty tons per day of chlorine dioxide.

The only death I am aware of was not due to an explosion in a chlorine dioxide generator, but in a chlorine dioxide solution storage tank in a pulp mill in Sweden. The tank had been emptied of chlorine dioxide solution for a piping repair, but the dilute chlorine dioxide gas remaining had not been blown out with air, as is the normal procedure. A welder put his oxy-acetalene torch to the pipe and the tank exploded and killed him.

How it came about

In late August, 1940, a telegram arrived at the Department of Chemical Engineering of the University of Toronto from Sigmund Wang, Research Director of Canadian International Paper Company, Hawkesbury, Ontario, indicating that they wanted to hire a Research Chemist. Professor Watson Bain, Head of the department, sent back a telegram naming three people: Alan Weatherburn; someone I can't remember; and me. I submitted an application and received an invitation to go to Hawkesbury for an interview. At the time I wrote the application, I was a full-time Demonstrator with a salary of twelve hundred dollars for seven months. I stated that my salary was one hundred and seventy dollars per month (without mentioning that I was only employed for seven months), and asked for two hundred dollars per month, or twenty four hundred dollars per year.

The interview

Between the time I applied for the job and went to Hawkesbury for the interview, I was promoted to Instructor, and my salary was increased to fourteen hundred dollars for seven months. Mary and I drove down to Hawkesbury for the interview. After being shown through the research laboratories and meeting Dr. Haskins, head of the Miscellaneous Research Department, who would be my boss, we were shown around Hawkesbury and the Annex, where the company houses were located. We met Pat Partridge, who had graduated in 1939 from our department, and his mother. We were quite impressed by the houses, and by the rent, which was twenty dollars per month for a six-room house.

Finally, I met with Mr. Wang and was offered the job. When we discussed salary, I told him that although I had asked for two hundred dollars per month in my application, I had been promoted at the University and raised to two hundred dollars per month there. I did not tell him that it was only for seven months of the year. He must have been impressed with me, because he immediately said: "We will pay you two hundred and fifty dollars per month." I was elated to receive a twenty-five percent increase in pay before I started work. However, I did not accept immediately because I wanted Mary's reaction, and I liked my new job at the university very much.

When I told Professor Bain that I was offered three thousand dollars per year, he thought that was an excellent salary. He said: "C.I.P. is a very good company, Rapson. If you stay with them and work hard, you ultimately can reach as much as five or six thousand dollars per year." In case this figure seems extraordinarily low today, remember we were just coming out of the great depression, assisted by the second world war, and his salary as head of the department was only forty-five hundred dollars per year.

Acceptance

Since I had completed my laboratory work for my Ph.D. and could write my thesis that winter, we decided to accept the job. We moved to Hawkesbury on October thirtieth, and I began work on November first, 1940. That winter we did not meet many people because I spent nearly all my evenings and weekends typing my thesis, and Mary had her hands full looking after the house and our baby, Margaret. We soon decided to stay only two years. After I got my Ph.D. in June, 1941, we began to enter more fully into the social life of the community; after two years we wanted to stay longer, and we stayed for twelve years.

The reserve army

After December 7, 1942, when Japan bombed Pearl Harbour and entered the second world war on the side of Germany, many of us in C.I.P. in Hawkesbury thought that Germany would ultimately attack Canada on the east coast and Japan would attack Canada on the west coast. Since we were across the Ottawa River from the foothills of the Laurentian Mountains, we wanted to form a guerilla unit to retreat into the hills and continue the fight from there. When we tried to do this, we were told that we could join the infantry and form a platoon of the reserve battalion of the Stormont and Glengary Highlanders based in Cornwall, with a branch in Alexandria. We could then get the weapons and training and develop the necessary skills to accomplish our goal of defending our homeland against the invaders.

During the war, chemists and engineers were not allowed to enlist in the regular army except when their special skills were needed for military purposes. We were kept at home to maintain the manufacturing industry support for the armed forces overseas. However, it was possible to do this and become a soldier in the reserve army. Virtually all the technical personnel in the mill and the research department signed up. One of our number, Edward Howard, had taken officer training in Montreal at the start of the war. He was, therefore, made the lieutenant of our platoon.

Three sections make up a platoon. Steve Partridge, the oldest among us, was named sergeant. Chosen as corporals were Pat Partridge, Bob Sherman, and I. Morris Wayman was made lance-corporal of my section. It was probably the only section in any army in the world which had two Ph.D.'s as corporal and lance-corporal leading a section of ten men! Our training was very serious. We went to Alexandria, Ontario, about twenty-five miles east of Hawkesbury, for training three nights a week. On Sundays we went out on maneuvers. We went occasionally to Cornwall for indoor rifle practice. Every summer we had to spend our two weeks holiday training at military camp at the Connaught Range north of Ottawa.

It was very hard on Mary, who had to look after our three children. They stayed at our cottage in the Laurentiens, near Hawkesbury, across the Ottawa River. They had no electricity and no telephone. Mary did not drive; so she was dependent on neighbours for ice and food and mail. It was part of the sacrifice everyone had to make for the war effort.

That camp was quite an experience. We slept in tents. The kitchen was a small building. The dishes were washed outdoors. Just before we went to camp, the first synthetic wetting agent, Nopco, had come to the lab for testing for resin removal in the caustic extraction stage of dissolving pulp purification. It worked so well that I took a small bottle of it to camp. When it became my section's turn to be on "KP" (Kitchen Patrol) and wash the dishes, I put my men in two lines at a table. Each line had a can for scrap removal, a washing bowl, and a rinsing bowl. I explained to them that the wetting agent would greatly improve the efficiency of washing and that I would inspect every dish as it came through. If there was any dirt on it, it would go back to the start of the line. This would encourage each man to do a good job and greatly speed up the operation to get us out of there faster. After a few rejections, the dishes came through cleaner than they ever had before. After cleaning all the pots thoroughly using the detergent, we were instructed to make ourselves scarce. We were not to be around for the camp inspection which had just begun.

I told my section that we would line up and march smartly down the road as if we were going somewhere officially. When there was no officer in sight, we would break off and lie on the grass in the shade of a tree. We would post a guard to look out for an officer. As soon as one appeared in the distance, we would swiftly form a line again and march smartly until he was out of sight and just as swiftly lie down again, posting another guard. Thus, we had an easy time until the next mealtime and simultaneously obeyed our instruction not to be seen hanging around. My section became very loyal supporters of Corporal Rapson.

The sad sequel to this was that the section which had KP the day before had not washed their pots, but had hidden them in a cupboard in the kitchen. Their inspecting officers did not find them, but ours did. We were blamed and had to wash double the number of dirty pots!

One thing was seared into my memory. On maneuvers one day, I asked Colonel Franklin (who later became Sergeant of Arms of the House of Commons) what we should do with our prisoners. He said: "We don't take prisoners, Corporal!" I was shocked, stunned! I hoped we would never have to go into battle!

Pistol shooting

Another event was lots of fun. We had been having target practice on the rifle range. When all had a turn, the Officers and NCO's were to move over to the pistol range for shooting practice. All the lower ranks were ordered back to camp for calisthenics. I spotted the ammunition truck near the pistol range. I whispered to Corporal Pat Partridge, my close friend: "Quickly, move over to the ammunition truck and stand at attention at the back of it!", which we did smartly. (One should always move smartly in the army, especially if one is up to some skulduggery.) Soon Sergeant-Major Fenton, in charge of the ammunition, came running over, shouting: "What in hell are you two doing here?" "We thought you would need some help passing out the ammunition!" He looked around, paused for a while, and said: "Well, since everybody else has marched back to camp, you may as well stay here and help me." Gurgling with inner joy, Pat and I jumped to get the ammo out of the truck and started distributing it to the Officers and NCO's as they moved up to shoot. There were seven targets for each group. As each competitor moved up to shoot, we handed him the appropriate number of bullets. We had a great time running about and watching the shooting. As Colonel Franklin moved up to shoot with the last group, he said: "These two men have been working very hard distributing the ammunition. There are two spare positions. Let them have a turn." Overjoyed, Pat and I smartly stepped up to shoot! We had never fired pistols before. Colonel Franklin was a good shot and beat all who had gone before. Pat had a higher score. What a debacle! Corporal Partridge, who was not supposed to be on the pistol range at all, had beaten all the Officers and NCO's. It shows what a little quick thinking can do!

Development of ClO_2 bleaching.

In 1940, when I went to Hawkesbury to the Research Department of Canadian International Paper Company, I had never heard of chlorine dioxide. I did not know that such a compound existed. In 1942, when chlorine was needed for the war effort, pulp mills were forbidden to bleach pulp to higher than seventy percent reflectance or "brightness", to conserve chlorine. All pulp bleaching was done at that time with chlorine, hypochlorite, and sodium hydroxide. With sulphite pulp it was easy to get to ninety brightness, especially for dissolving pulps.

The C.I.P. Research Director, Mr. Sigmund Wang, called me into his office and said: "We want to find some chemical which is not needed for the war effort, which will bleach pulp to ninety brightness without using chlorine." Therefore, I investigated the bleaching effect of every oxidizing agent I could think of. I tried hydrogen peroxide, sodium peroxide, sodium perborate, ammonium persulphate, sodium chlorite, and chlorine dioxide. Of these, only sodium chlorite and chlorine dioxide would bleach pulp to ninety brightness.

Sodium chlorite is made from chlorine dioxide by reacting it with sodium hydroxide and hydrogen peroxide, reducing it to chlorous acid, $HClO_2$, which in the alkaline solution becomes sodium chlorite, $NaClO_2$. Since production of sodium chlorite requires chlorine dioxide plus these other chemicals, sodium chlorite can never be as cheap as chlorine dioxide itself. One of its five oxidizing equivalents is lost in the process and the cost of the other chemicals and the processing must be added for the four remaining oxidizing equivalents.

Obviously, chlorine dioxide was the most suitable candidate. In the laboratory, we made ClO_2 from $NaClO_2$ and Cl_2 by the following reaction:

$$2NaClO_2 + Cl_2 \Rightarrow 2ClO_2 + 2NaCl$$

During the investigation of bleaching sulphite pulp with chlorine dioxide, we discovered that chlorine dioxide in the first stage of bleaching has a very significant effect. It lowers substantially the resin content of the pulp after the caustic extraction stage and, therefore, in the final pulp.

At that time there was a very serious shortage of softwoods in the neighbourhood of Hawkesbury, because they had been harvested. Sprucewood was being hauled by rail from as far away as New Brunswick; so it was very expensive. Plenty of hardwoods were available in the neighbourhood of Hawkesbury mill; they had been left behind when the softwoods were removed and they propagate very rapidly. Ultimately, it would be necessary to use hardwoods in Hawkesbury mill. However, hardwoods left a lot of wood resin in the dissolving pulp, and this had a deleterious effect in the manufacture of viscose rayon for which the pulp was used. Therefore, it would be very important to substitute chlorine dioxide for chlorine in the first stage of bleaching hardwood dissolving pulp. We quickly showed in the laboratory that this was so. Chlorine dioxide would substantially lower the resin content of hardwood sulphite pulp, especially for birchwood, the most suitable hardwood pulp because of its longer fibre length (about one and one-half millimeters compared with other hardwood pulps such as maple and oak with one millimeter or less average fibre length).

Unfortunately, chlorine dioxide cannot be shipped, because it is an explosive gas, with low solubility in water. Whenever concentrated in air or other gas above about ten percent by volume, chlorine dioxide spontaneously explodes, after a certain time ("induction period"), which is shorter the higher the concentration and the higher the temperature. Therefore, it must be produced at the site of use, diluted with about ten times its volume of air and then dissolved out of the air into water to produce an eight to ten grams per litre solution, which is stored in a tank and used for bleaching.

To try out this new discovery in the mill, it would not be practical to build a chlorine dioxide plant. Furthermore, a process for manufacturing chlorine dioxide was not available. However, chlorine dioxide can be made from sodium chlorite and chlorine. This is an expensive way to make chlorine dioxide, because of the cost of chlorite as discussed previously, but it is a very simple way to obtain chlorine dioxide. Therefore, for a mill experiment at Temiskaming we purchased sodium chlorite. It was only necessary to dissolve the chlorite in water and add it continuously to the mixer in which chlorine was being added to the flowing stream of pulp suspension going into the chlorination tower in the normal way. Instantly chlorine dioxide was produced in the solution in the mixer, which at that time was only an oversized pump because the consistency was only three percent pulp in water.

At that time, sodium chlorite was one dollar per pound, compared with three cents per pound for chlorine. For a mill scale test of an important improvement in bleaching technique, such a cost was fully justified. The mill quickly confirmed our laboratory results on the lowering of the resin of the dissolving pulp.

Chlorine dioxide generation

No method for on-site manufacturing of chlorine dioxide was available at that time. Therefore, we set out to develop a method for producing chlorine dioxide at our mills. I studied all the literature on chlorine dioxide. I went to the University of Toronto Library to complete the survey of all literature on the subject. After studying all the possible processes for making chlorine dioxide, I concluded that the method patented by Cunningham of Mathieson Alkali Company of Niagara Falls, N.Y., in 1938, using sodium chlorate solution in a tank through which sulphur burner gas (containing eighteen to twenty-one percent SO_2 and the rest mainly nitrogen) was passed through a sparger. The sulphur dioxide rapidly reacts with the chlorate to produce chlorine dioxide and sodium sulphate. The sulphur dioxide is oxidized to sulphur trioxide, which reacts with the water present to produce sulphuric acid. The chlorine dioxide produced is removed by the nitrogen and used by Mathieson Chemical Company to make sodium chlorite. This was a batch process which appeared to be cumbersome and difficult to handle in a continuously operating bleach plant.

Therefore, I conceived of a simple way to make this batch method into a continuous one by passing a sodium chlorate solution, as concentrated as possible, about six hundred grams per litre, down a packed column counter-current to a stream of sulphur burner gas passing up the column. The sulphur dioxide reacts with the chlorate to produce chlorine dioxide, sodium sulphate, and sulphuric acid in the solution which exits from the column. The chlorine dioxide diluted with nitrogen (or air, if purchased sulphur dioxide is used instead of sulphur burner gas) passes through piping to the bottom of the packed absorption tower up which it passes counter-current to cold water passing down the column to dissolve the chlorine dioxide. The whole process is operated by vacuum on the top of the absorption tower, which prevents any obnoxious gas from escaping to the atmosphere.

Morris Wayman and I operated this process on a bench scale with one-inch diameter columns and on a pilot plant scale in two-inch diameter glass columns in April, 1945. It worked so well that we immediately ordered fifteen-inch diameter stoneware columns and auxiliary equipment for the mill-scale plant for the one hundred tons per day pulp mill in Temiskaming. Morris Wayman and I started up this first commercial chlorine dioxide plant on June first, 1945. The chlorine dioxide solution was intended to pass to a storage tank, from which it was to be distributed through pipes to the bleaching equipment as required.

However, the stoneware storage tank had not arrived by the time we started up the plant. Therefore we operated straight from the chlorine dioxide plant, through a 400-foot long glass pipe into the chlorination tower in the bleach plant. Since the mill was built on a hill with the digester building at the top, gravity flow in the glass piping through the digester room and screen room was possible; this avoided the need for a chlorine dioxide solution storage tank. When the tank finally arrived, it was used for storing chlorine dioxide solution in the Research Department in Hawkesbury.

Although this is an extremely simple system, very easy to operate, it was not very efficient. Only about seventy percent of the theoretical yield of chlorine dioxide from the chlorate was obtained. The reason for the inefficiency was that sulphur dioxide and chlorate are being consumed in producing the sulphuric acid necessary for the process to work. We found that adding sulphuric acid with the chlorate solution would increase the yield substantially, but the inconvenience and the cost were not justified.

Just after the one ton per day chlorine dioxide plant started up, CIL., the supplier of chlorine to the Temiskaming mill, had a strike and the mill had to stop producing pulp. The mill manager called me into his office.

"Can this new chlorine dioxide plant of yours supply enough chemical to keep the small unit (one hundred tons per day of bleached sulphite pulp) running?"

"Yes!" I said, "as long as either Dr. Wayman or I stay here to supervise it."

"Good!" he said, "Please do that. It will allow us to continue making one hundred tons per day of pulp."

Therefore, Morris and I took two week turns with our families in Temiskaming. The strike lasted nine weeks. The mill made about six thousand tons of pulp which could not have been made if we had not been able to produce chlorine dioxide. This more than paid for all the research, the development, and the capital cost of the development of chlorine dioxide, both for making it on site and for bleaching with it.

KAREL WIESNER

(November 25, 1919 – November 28, 1986)

Killam Laureate, 1986

Karel Wiesner was born in Prague, Czechoslovakia, the only son of a prominent engineer industrialist, Karel Wiesner, and his wife, Eugenie (Sterova) Wiesner. He studied at the Prague Institute of Technology (Chemical Engineering) and at Charles University in Prague.

He received his doctorate in Physical Chemistry in 1945, in Professor Heyrovský's department, for research performed without direction during the war — when Czech Universities were closed — in the Research Department of the Bulovka Hospital. After receiving his degree, Wiesner spent the year 1945-46 at Charles University as Assistant in the Department of Physical Chemistry. In 1946 he left for Zürich and worked with Professor V. Prelog in Professor Ružička's Laboratory at the Federal Institute of Technology for two years. In 1948 Wiesner came to Fredericton, with his wife Blanche and son Charles, to take up duties as Professor of Chemistry at the University of New Brunswick.

In New Brunswick he started a school of Natural Products Chemistry and worked with a large group of students first on the structure determination and later on the total synthesis of very complex natural products of novel type.

From 1960 to the time of his death, Wiesner was associated with the Ayerst Research Laboratories in Montreal and actively involved in drug research. He spent the years 1962-64 in Montreal at the Ayerst Research Laboratories as Associate Director of Research. In 1964 Wiesner returned to U.N.B., where he held the position of University Professor.

In his early work in Prague during the war, Wiesner discovered polarographic kinetic currents and elaborated a simple mathematical theory of this new type of polarographic waves which was cited in Professor Heyrovský's Nobel lecture. This simple treatment was later found to be essentially correct by rigorous calculations. Polarographic kinetic currents were historically the first method which made possible (in suitable cases) the determination of the rates of extremely rapid reactions, previously regarded as instantaneous. Of the various applications, two were elaborated by Wiesner at the University of New Brunswick.

The first involved the determination of all rate and equilibrium constants in the mutarotation equilibrium of glucose

$$\alpha \text{ glucose} \underset{k_1'}{\overset{k_1}{\rightleftharpoons}} \text{aldehyde glucose} \underset{k_2'}{\overset{k_2}{\rightleftharpoons}} \beta \text{ glucose.}$$

The value for the equilibrium concentration of aldehyde glucose derived from this work has, in recent years, been corroborated by direct measurement using circular dichroism (see I.D. Hayward and S.J. Angyal, Carbohydrate Research 53, 13-20 (1977)).

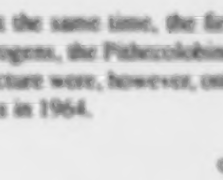
The second involved the determination of the rate of the second "instantaneous" step in an SN_1 displacement

$$R\text{-}X \xrightarrow{k_1} R^+ \xrightarrow{k_2} R\text{-}S.$$

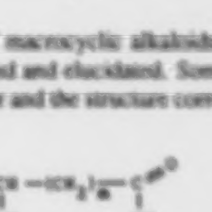
This second case is interesting since it is an application of the rate controlled current method to a system which is not in equilibrium. Thus, the relaxation methods developed later by Eigen cannot be used, and polarographic kinetics is the only technique capable of yielding the rate of the fast step. Wiesner has remarked that his polarographic studies would not have been possible without the assistance of his father who purchased and arranged shipment of essential equipment from outside Czechoslovakia during the difficult war period.

During the last three years of the war, Wiesner was employed by the Pharmaceutical Company B. Fragner in Prague. He led a team in the isolation and large scale production of quite pure penicillin. It went under the name B.F. 510 and was injectable without side effects. The information available in Prague was only the original paper by Fleming. Fleming's cultures, available to the Oxford group, were, of course, unobtainable in Prague. B.F. 510 was supplied, without the knowledge of the Germans, to Czech hospitals in Prague.

In the field of structure determination of complex structures of novel type the following are the most noteworthy examples. In 1953 the structures of the first two diterpene alkaloids, Veatchine 1 and Atisine 2, were determined.



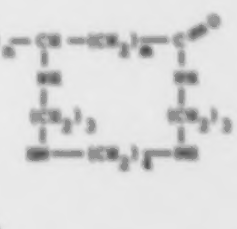
1



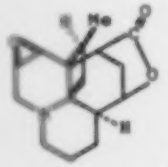
2

At about the same time, the first examples of macrocyclic alkaloids containing four nitrogens, the Pithecolobines 3, were found and elucidated. Some details of this structure were, however, only clarified later and the structure corroborated by synthesis in 1964.

3a, n = 3, x = 6
3b, n = 1, x = 8

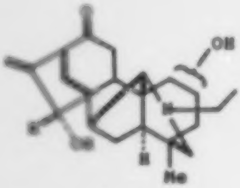


In 1957 the structure of the first Lycopodium alkaloid, Annotinine 4, was determined.



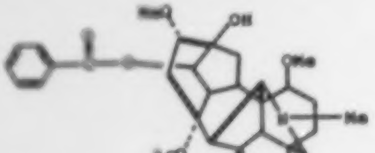
4

Shortly after this, the structure of the first ring B bridged diterpene alkaloid, Songorine, isolated from the mother liquors of Aconitine, was determined as 5.

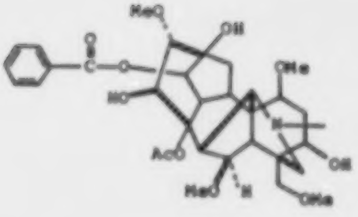


5

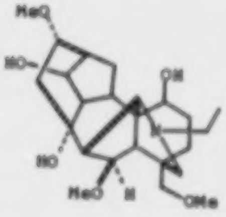
This was quite important, as Songorine is the simplest of the "complicated" diterpene alkaloids, and its structure was of considerable help in later more difficult problems. The ring A hydroxyl was later shown by Japanese workers to be located at C_1. After the elucidation of Songorine, the stage was set for an attack on the most complex representatives of the diterpene alkaloids.



6

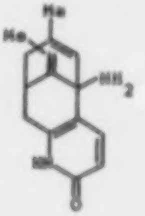


7



8

Thus Delphinine 6 and Aconitine 7, classical targets of structural studies for one hundred years, were elucidated in 1959, and Neoline 8, a congener of Songorine, was clarified in 1960. The studies on Aconitine were performed jointly with the group of Professor G. Büchi at the Massachusetts Institute of Technology. In the year 1960, the second Lycopodium alkaloid, Selagine 9, was also elucidated.

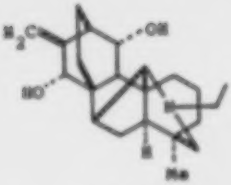


9

This work was entrusted to Wiesner by the Merck Institute of Medical Research in Rahway, since Selagine was found to display interesting pharmacological properties. It was the first of several subsequent cases where Industry paid the expenses of interesting structural or synthetic studies, the results of which were normally published and provided graduate students with good training.

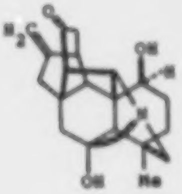
During the two year full time period at Ayerst, Wiesner initiated a project which involved the isolation and pharmacological screening of various natural products.

It was a strange coincidence that the structure of Denudatine 10 was elucidated in the course of these studies.

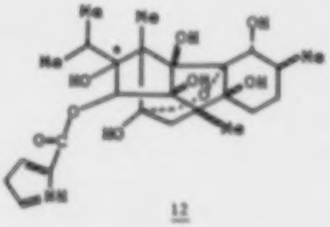


10

This is quite important since compounds with the skeletal structure 10 were predicted by Wiesner as biogenetic intermediates in the biogenesis of alkaloids of the delphinine type. This prediction was actually made in 1956. Another interesting compound which came out of this program was Delnudine 11, elucidated by chemical and X-ray methods. The interesting and unprecedented biogenesis of this compound was also pointed out. In conclusion of a brief survey of structural studies conducted by Wiesner and his students, Ryanodine must be mentioned. This potent insecticide was under experimental investigation from 1951 to 1962. Finally, in the fall of 1966, Wiesner succeeded in rationalizing the large amount of experimental data and proposed the structure 12 for the compound.



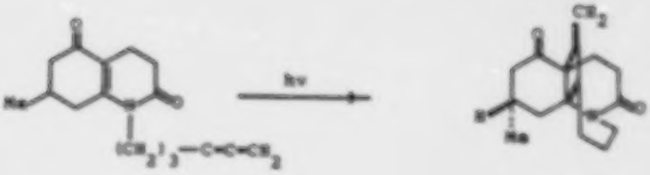
11



12

This structure was corroborated a year later by X-ray crystallography. The X-ray study also revealed the undetermined stereochemistry of the carbon carrying the isopropyl group (marked by the asterisk). Ryanodine is a diterpenoid of novel type. Nevertheless, its biogenesis seems to be normal and is based on a cyclization of geranyl-geraniol followed by secondary oxidations.

A new photochemical reaction designed for this specific purpose enabled Wiesner to synthesize Annotinine 4 in 1967. This was the first total synthesis of a natural Lycopodium alkaloid. The same reaction performed as a photocyclization made possible an exceedingly simple synthesis of Epilycopodine 13.



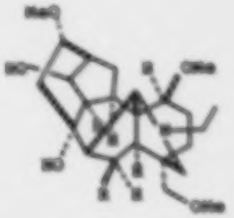
13

From his various studies at Ayerst Laboratories or sponsored by Ayerst at the University of New Brunswick, the most notable accomplishments were novel drug syntheses which included equilin, cardiotonic lactones, isomorphine, and prostaglandin derivatives. The preparation of desoxyprostaglandin in 1966 constituted the first synthesis of a fully active prostaglandin derivative.

Since his return to the University of New Brunswick from Ayerst in 1964, Wiesner was engaged almost exclusively in developing photochemical synthetic methods, novel in many cases, for the simple synthesis of complex compounds. The idea underlying all this work was to construct complex polysubstituted compounds in such a manner that all the substituents materialize automatically in the correct positions and do not have to be laboriously introduced.

Thus Veatchine 1 was synthesized in 1964 and a different much more simple photochemical synthesis of this alkaloid was designed by Wiesner in 1968. Atisine 2 was synthesized by a simple method which included two completely novel reactions in 1966.

Following a number of other outstanding accomplishments in organic synthesis, Wiesner achieved a major milestone in providing the first total synthesis of the exceedingly complex Delphinine-type alkaloids Talatisame 14 and Chasmanine 15.



14, R = H
15, R = OMe

Observations by Wiesner in the course of the various photochemical syntheses of Lycopodium and diterpene alkaloids led him to the discovery of the first case in which sigma bond electrons were found to be part of an electron system giving rise to an ultraviolet spectrum. Subsequent cases were discovered by Cookson in England, and the phenomenon is now regarded as fully established.

In the 1980's, Wiesner decided to change his focus and to concentrate on the study of digitalis, the steroid cardioactive drug currently used to regulate human heartbeat. With the aim of decreasing its toxic side effects, which often proved fatal over time, he synthesized many derivatives of the drug. In 1983 Wiesner and his research team made a major breakthrough and were able to show that the desired cardiotonic activity and the toxic activity of digitalis formulations are manifest at different receptor sites. Following up on this lead, his research group was able to prepare digitalis derivatives with a much improved ratio of therapeutic value to toxicity and paved the way for potential major development in heart drug therapy.

Wiesner's early structural studies were pursued with the assistance of a team of devoted graduate students, largely drawn from New Brunswick, who were inspired by his teaching and enthusiasm for his work. As the reputation of the Wiesner school spread, increasing numbers of international scholars were drawn to the University of New Brunswick. He taught by example and preferred informal discussions in the laboratory to formal lectures. Most importantly, he was always able to infect everybody who worked with him with his enthusiasm and scientific curiosity.

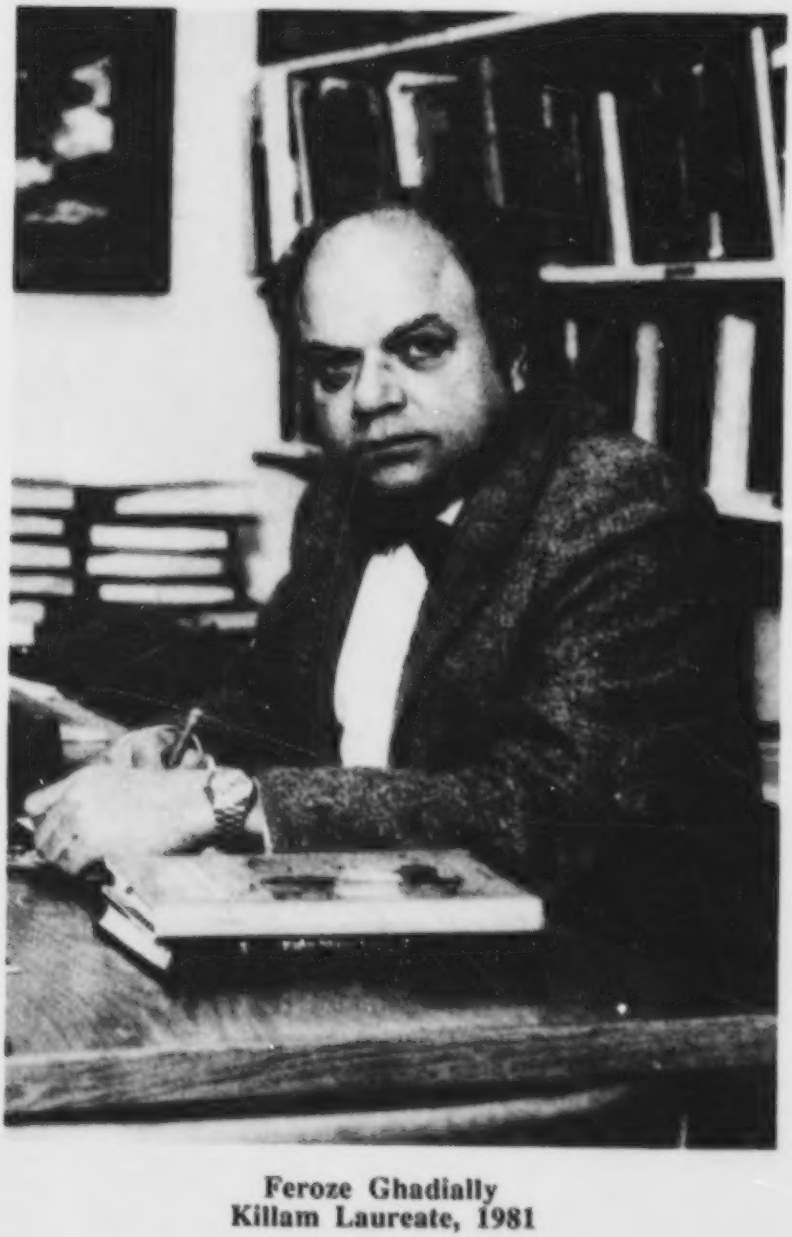
What characterized his work was that he invariably selected problems, structural or synthetic, which were seemingly impossible to solve with the tools available at the time. His supervision was frequent and often highly educational. A story can be recalled in which a cautious student asked him if it is not too daring to tackle problems which seem virtually insoluble. After a brief throught, Wiesner, a keen duck hunter, smiled and answered: *"You know, you will never bag a tiger if you insist on chasing after rabbits!* "

Whatever Wiesner undertook, whether in science or sport, he did so with determination and devotion; thus, in his youth in Czechoslovakia, he developed a passion for tennis which led him to become a player of national repute and he remained an avid fan of the game throughout his life.

In his nearly forty years' association with the University of New Brunswick, Professor Wiesner supervised over eighty theses and trained and encouraged a myriad of graduate students, post-doctoral fellows, and research associates who, in turn, have made contributions in natural products chemistry, both structural and synthetic, at numerous institutions around the globe. He authored nearly two hundred scientific papers from 1942 to 1986 and delivered lectures on his many fields of interest in Canada, the United States, the United Kingdom, and Europe.

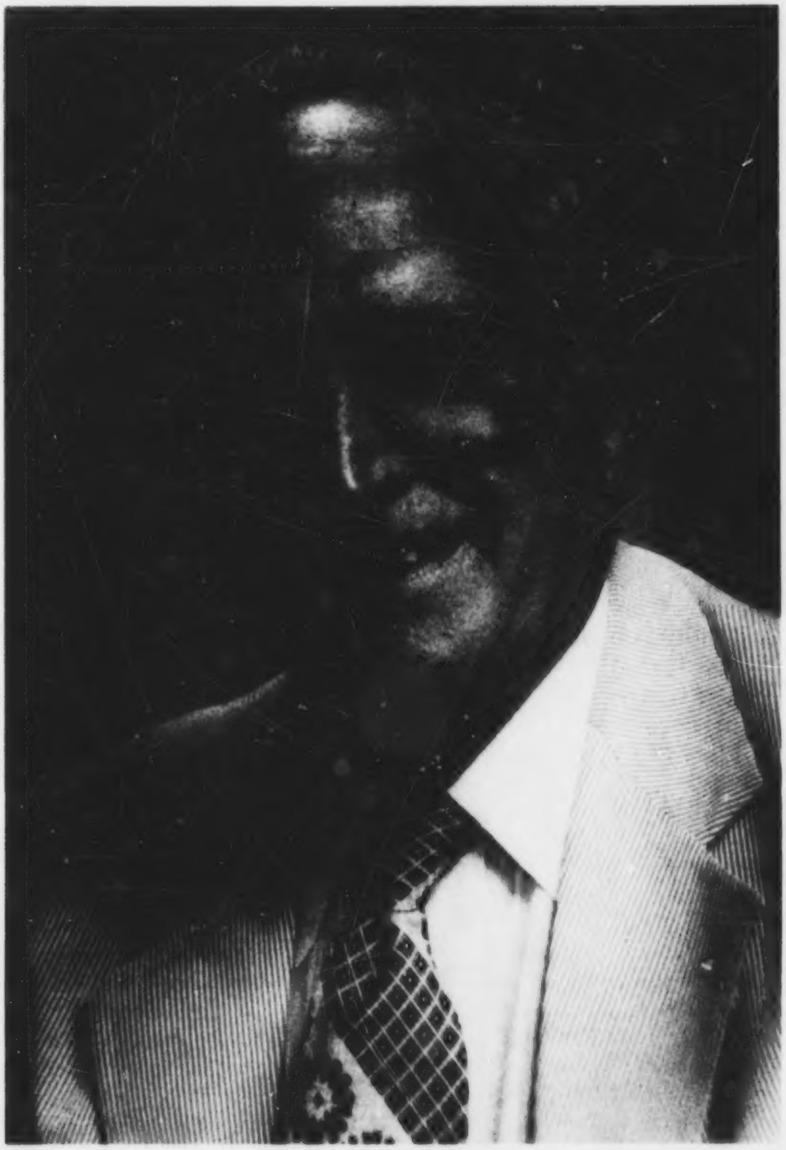
In recognition of his many contributions, Karel Wiesner received numerous honors during his career. In 1957, he was elected to the Royal Society of Canada, and in 1969 he became a Fellow of the Royal Society of London. In 1963, he received the Palladium Medal, the highest honor of the Chemical Institute of Canada. Three institutions conferred honorary doctor of science degrees upon him: the University of New Brunswick in 1970, the University of Western Ontario in 1972, and the University of Montreal in 1975. He was awarded the Centennial Medal of the Royal Society of London in 1967, the Drinov Medal from the Bulgarian Academy of Sciences in 1978, and the Order of Kyril and Methodium First Class by the Bulgarian Government in 1981. In 1975, he was appointed Officer of the Order of Canada and was awarded the Ernest Guenther prize by the American Chemical Society in 1983. In 1976, Karel Wiesner was named University Professor, the highest honor the University of New Brunswick accords its professors.

In 1978, Karel Wiesner was elected a member of the Pontifical Academy of Sciences, Rome. The seventy-member academy, founded in 1603, meets regularly to discuss matters of scientific and humanitarian concern. In 1985 — the year before he died — Professor Wiesner became the first scientist in the Maritimes to receive the Izaak Killam Memorial prize for distinguished lifetime achievement.



Feroze Ghadially
Killam Laureate, 1981

Raymond Lemieux, Killam Laureate, 1981
(receiving an honorary degree from the University of Stockholm)



Louis Siminovitch
Killam Laureate, 1981

William Tutte
Killam Laureate, 1982

Brenda Milner
Killam Laureate, 1983

Werner Israel
Killam Laureate, 1984

Pierre Dansereau
Killam Laureate, 1985

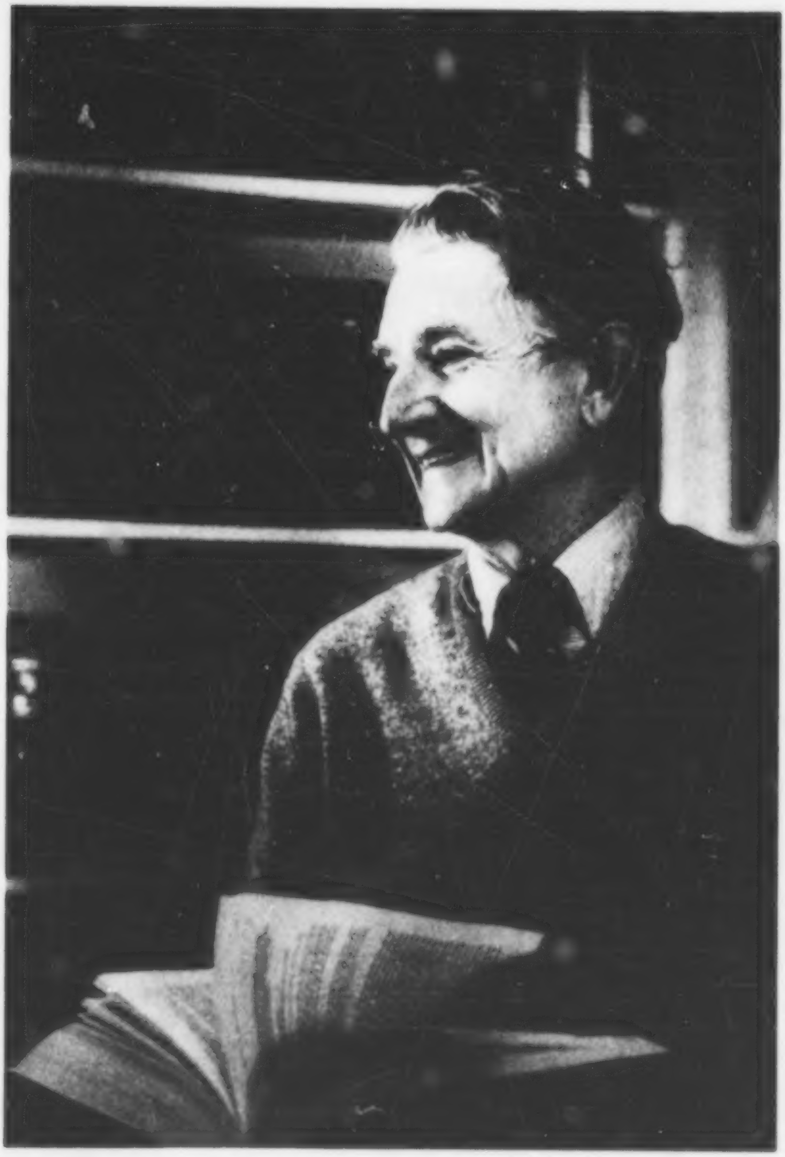
Phil Gold
Killam Laureate, 1985

Ralph Gordon Stanton
Killam Laureate, 1985

Raymond N. Yong
Killam Laureate, 1985

Jacques Genest
Killam Laureate, 1986

Howard Rapson
Killam Laureate, 1986



Karel Wiesner
Killam Laureate, 1986

Ronald James Gillespie
Killam Laureate, 1987

James Fraser Mustard
Killam Laureate, 1987

Ashok Vijh
Killam Laureate, 1987

Henry J.M. Barnett
Killam Laureate, 1988

William Henry Gauvin
Killam Laureate, 1988

John Polanyi
Killam Laureate, 1988
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J. Keith Brimacombe
Killam Laureate, 1989
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Jules Hardy
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J. Tuzo Wilson
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J. William Costerton
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M. Daria Haust
Killam Laureate, 1990

Sidney van den Bergh
Killam Laureate, 1990

RONALD JAMES GILLESPIE

(August 21, 1924–)

Killam Laureate, 1987

I was born in London, England, the first of three children, two boys and a girl, of James Andrew Gillespie and Miriam Kirk. My father worked in the Post Office in London, first as a sorter and then as a clerk. In 1936, I passed the 11+ entrance examination and spent the next six years attending Harrow County Grammar School. Secondary education was not entirely free in those days, and my parents made considerable financial sacrifices to ensure that all three of us obtained the best education possible for children of parents of limited means. I did not do well enough in the entrance examination to enter the "classics" stream in the grammar school; so I was placed in the science stream, where I studied chemistry and physics instead of latin and general science, in addition to the other usual subjects. After "matriculation", I moved rather naturally into the science sixth form and spent two years taking physics, chemistry, pure maths and applied maths. This highly specialized course was normal at that time in Britain, but I now regret that, at a time when my interest in literature, history, and foreign languages was only just beginning, I never had the opportunity to continue my studies in those areas.

On the results of the Higher Certificate Examination, I was awarded a wartime State Bursary in 1942 to take chemistry at University College, London, then evacuated to the University of Wales in Aberystwyth. The B. Sc. Honours Chemistry at that time was an abbreviated "wartime" two-year course which gave one temporary exemption from military service. I chose to go into chemistry largely because of the influence of the chemistry master who, in contrast to the physics master, did not just read the textbook to us but left us to read the text-book while he explained chemistry to us and challenged us to work things out for ourselves both in the class room and in the laboratory. Moreover, working in the chemistry lab with solutions, precipitates and color changes and the puzzles of qualitative analysis was much more fun than playing with springs and meter rules and clocks in the physics lab. The same teacher also inspired William Moffit, another member of my class, to take up chemistry. Moffit later worked with Coulson and became a professor of Theoretical Chemistry at Harvard. I also chose chemistry because I discovered that I was no mathematician. I spent many hours on my maths homework only to find that the real mathematicians like Moffit had done the same problems in ten minutes. Conscious of our priviliged position, most of my colleagues and I worked very hard at the intensive B. Sc. course, although I did have time to discover the pleasures of the Welsh mountains and to continue to compete in track and field and in cross country running which I had started in school. Rather to my surprise, I gained a first-class degree and was second in the University of London in Honours

Chemistry. In retrospect, I realize that I worked very hard during those two years because I had a real desire to understand science, and chemistry in particular, and I obtained a great satisfaction from gradually increasing my understanding.

On graduating in 1944, I was offered the chance to stay on at University College for a Ph. D. degree under the supervision of Professor C. K. Ingold. Of course I accepted, but it is interesting to look back now and to realize that the possibility of perhaps transferring to another institution was never even mentioned or considered. At that time, before the end of the war, and somewhat prematurely as it turned out in view of the advent of the flying bombs, University College moved back to the William Ramsay and Ralph Forster Laboratories on the campus of University College in London. They had been largely unoccupied for several years and were surrounded by bomb damage all around. They were dusty, dirty, and ill-equiped, but we knew nothing better and somehow research got underway. My problem was to study the ionization of nitric acid and the oxides of nitrogen in 100% sulfuric acid by cryoscopy or freezing point depression measurements. This was part of a much wider programme in Ingold's laboratory which was concerned with the mechanism of aromatic nitration. This research was supposed to be of military interest because of its possible importance to the production of explosives. Ingold was one of the leading figures in the field of physical organic chemistry, and so I had considerable exposure to this field. Most of my fellow students were measuring reaction rates by watching the rise or fall of a liquid surface in a dilatometer, which seemed rather dull to me, so I was pleased to have my own different and independent problem. One of these students evidently also thought that these reaction rate measurements were as dull as I did, as he eventually became a professor of botany at University College. As I studied the ionization of nitric acid and the oxides of nitrogen in sulfuric acid, I became increasingly interested in its properties as a solvent, and I extended my measurements beyond the original project to include nitrocompounds, carboxylic acids, and other organic compounds, and also to the physical properties of sulfuric acid itself.

In 1948, before I had written up my Ph. D. thesis, I was offered a position as Assistant Lecturer (at a salary of £500 a year!). It seems remarkable now, when I look back on it, that I was only 24 at the time and I had a post without ever having made a single application. I was particularly fortunate, I think, that Ingold allowed me and, as I realize now, encouraged me to develop my own interests while I was still working on my thesis project. I thus gained confidence in my ability to work independently and to choose whatever project fascinated me most and to generally plan my own work. Looking back, I realize that I have rarely been involved in collaborative projects or attempted to follow any particular fashionable trend. In other words, I have always had the good fortune to be able to do my own thing. Of course I was fortunate that, under the British system at that time, I never needed to look for my own funds—my modest needs at that time were supplied by the head of the department, and so what I chose to do was not influenced by what I could get funds for. My background thus probably explains why I am so opposed to the current trend to preferentially support so-called "relevant" research. I strongly believe that those who have demonstrated their ability to do good science are the best judges of what they should work on. There is now far too much emphasis here in Canada, as well as in other countries, on the nature and "relevance" of one's proposed project as judged by a committee and far too little on one's earlier accomplishments, as used to be the case during the regime of the NRC as a grant giving body.

Soon after I had been appointed to the position of Assistant Lecturer, several graduate students were assigned to me. I say "assigned" because neither they nor I had any choice in the matter. This enabled me to rapidly expand my studies of sulfuric acid. I started systematic studies of the conductivity of sulfuric acid solutions, which were of particular interest because the same type of chain mechanism that is important for the abnormally high conductivities of H^+ and OH^- in water is even more important for the conductivities of the analogous acid and base ions $H_3SO_4^+$ and HSO_4^- in sulfuric acid. Then we showed that the very high conductivities of these two ions make conductivity measurements useful as a supplement to cryoscopic measurements for the study of the ionization of solutes in sulfuric acid. We also studied the rather complex self-ionization equilibria in the pure acid; at the same time, I became interested by the fact that solutions in sulfuric acid appeared to behave more ideally than solutions in water. This latter discovery led to a study of activity coefficients and to the conclusion that sulfuric acid must have a very high dielectric constant. This work represented one of the most complete studies of a non-aqueous solvent system that had been made at that time. An important part of this work was carried out by Dr. E. A. (Peter) Robinson, now at Erindale College and a former principal of that college, when he was a graduate student with me in London. At that time I seemed to be well on the way to becoming a physical chemist.

In 1954 I obtained a Commonwealth Fund Fellowship (now known as a Harkness fellowship) to go to Brown University, Rhode Island, to work with Robert Cole, a well-known expert in the field of dielectric measurements, on the dielectric constant of sulfuric acid. I spent nine months at Brown, during which time we succeeded in showing that sulfuric acid does indeed have a very high dielectric constant of 120 at 10° C, although the measurements turned out to be rather difficult because of the very high conductivity of 100% sulfuric acid. This was my first experience of an institution other than University College, of a department where there was not a heavy emphasis on physical organic chemistry, and of a completely different, more informal, life-style both in and out of the laboratory. In the summer, my wife joined me and we spent three fascinating months touring the U.S. by car from coast to coast and from north to south, visiting many of the major national parks. Clearly this very educational and enjoyable experience set the seeds for our later move to Canada.

But I was not destined to become a physical chemist. Having solved the problem of the dielectric constant of sulfuric acid and its self-dissociation equilibria etc. , there seemed little more that could profitably be done on its physical properties. I turned to inorganic chemistry and I have remained in this field ever since. My interest in inorganic chemistry, which first started when I was an undergraduate, despite an immensely dull text-book by Ephraim and (perhaps because of) an eccentric but charming lecturer named Henry Terry who pulled his lecture notes out of his pocket on odd scraps of paper and old envelopes, had already been stimulated by my studies of nitric acid and the oxides of nitrogen and a much more interesting new text-book by Emeleus and Anderson. And I had also worked on solutions of Cl_2O in sulfuric acid in an unsuccessful attempt to generate the Cl^+ ion, which was much beloved by the physical organic chemists at that time as the active intermediate in aromatic chlorination. During my reading of the literature of the early work on solutions in sulfuric acid, I had become intrigued by the fact that certain elements, in particular, iodine, sulfur, selenium, and tellurium, dissolve in concentrated or 100% sulfuric acid to give brightly colored solutions whose nature was not at all well understood. So I set out to study some of these solutions and to find out what the highly colored species were. This turned out to be an important decision, as this investigation occupied the attention of my research group for many years. It was also becoming clear that the usefulness of sulfuric acid as a solvent for generating new and interesting species was due to its very high acidity or low basicity which allowed one to generate species that are not stable in water, which is a much less acidic and therefore much more basic solvent. So I also set out to extend the earlier measurements of the physical organic chemist Hammett on the acidity of H_2SO_4–H_2O mixtures to 100% H_2SO_4 and beyond into oleum, that is, H_2SO_4–SO_3 mixtures. And I also began to think about the possibility of other still more acidic media. By replacing an OH in H_2SO_4 by the more electronegative F, it seemed that one should generate a stronger acid. Fluorosulfuric acid, HSO_3F was known, but had been little studied at that time. Our subsequent studies of this acid and its use as a solvent for generating new species eventually led to the concept of superacids and their extensive use in organic chemistry, for example, for generating stable solutions of carbonium ions.

Although I was thus becoming an inorganic solution chemist, I had also developed an interest in structural inorganic chemistry. This interest stemmed partly out of the fact that the first course that I was assigned to teach was called Molecular Properties. It was concerned with bond lengths, bond angles, bond energies, dipole moments, etc. Later I was also assigned a course on Quantum Mechanics and the Chemical Bond, and this did much to enhance my understanding of the fundamentals of molecular structure. My interest in structural inorganic chemistry was further enhanced by the arrival in the department at University College of several inorganic chemists from Australia as postdoctoral fellows. Prominent among them was R. S. Nyholm who became internationally known for his work in transition metal chemistry and chemical education and became Sir Ronald Nyholm and Professor of Inorganic Chemistry at University College. He and I were both dissatisfied with the conventional explanations for molecular geometry that we gave in our courses. As a result of many discussions on the subject, we wrote an article in 1957 in the Quarterly Reviews of the Chemical Society called "Inorganic Stereochemistry" in which the foundations for what is known as the VSEPR model were laid. Because I moved to Canada a year later and because Nyholm was most unfortunately killed in an accident a few years later, we did not collaborate any further on the VSEPR model, but I continued to develop it. It gradually became the most widely used model for teaching molecular geometry at an elementary level, and it has also proved very useful to inorganic chemists for discussing the structures of many new molecules. In 1972, I published a book called "Molecular Geometry" which dealt with the VSEPR model and with its many applications. This book is now out of print and also out of date; so I am now just completing a very extensive revision, because there are now so many interesting new examples of the application of the VSEPR model and we also have a much better understanding of it. Indeed, very recently I have been collaborating with my colleague Richard Bader to use his ideas relating to the calculated electron density in a molecule to provide a real physical basis for the VSEPR model.

Although they were exciting times at University College after my return from the U.S., I became increasingly dissatisfied with the hierarchial system and with the limitations imposed by having no personal research funds and very little modern equipment. I was still operating largely with the classical methods of cryoscopy, electrical conductivity, and u. v. and visible spectroscopy, but the need to utilise X-ray crystallography, vibrational spectroscopy, and nmr to determine the structures of the new species that we were making was becoming apparent. The department did have X-ray crystallographic equipment and a Raman spectrometer, but access to this equipment was difficult because it formed an essential part of other research projects. So, after Art Bourns from McMaster had spent a sabbatical year at University College, I wrote to him to ask about openings in Canada. The result was that I was offered a position at McMaster that I took up in 1958 despite strong persuasion to stay at University College. Peter Robinson came with me as a post doctoral fellow, and together we set up a Raman spectrometer. We also soon purchased one of the early model Varian nmr spectrometers. Jim Barr, now at Mohawk College, came with me to complete his Ph. D. on solutions of selenium in sulfuric acid, and Bob Thompson from Western Ontario, and now at UBC, was my first Canadian graduate student. The sense of freedom that came with having my own research funds and new equipment under my control, together with the excitement of making a fresh start, gave my research effort a big boost and we moved quickly into an extensive study of more strongly acidic solvents, such as HSO_3F and HF, that are now known as superacids. We found that the addition of Lewis acids such as AsF_5 and SbF_5 to HSO_3F and HF greatly enhanced their acidity, and we were thus able to make and measure the acidity of the most highly acidic systems that have yet been prepared. These superacid systems have found widespread application for the protonation of very weak organic bases and the preparation of stable solutions of carbonium ions. We made extensive use of them to develop our studies of the cations of the halogens and the chalcogens that we were then discovering were the origin of the brightly colored solutions in sulfuric acid that had been reported many years before. That these nonmetallic elements could be obtained in the form of polyatomic cations such as $I_2{}^+$, $S_8{}^{2+}$ and $Se_{10}{}^{2+}$ opened up a whole new field of main group chemistry and our studies of the preparation and structures of these cations, using principally Raman and nmr spectroscopy and X-ray crystallography, extended over many years. Neither the existence nor the structures of these species could, I believe, have been predicted. A whole new important field of inorganic chemistry arose simply from a desire to find out what the intriguing colors of solutions of iodine and sulfur in sulfuric acid were due to. And accounting for the structures of these species continues to exercise our imaginations. We extended these studies to include the preparation and structure determination of several new sulfur-nitrogen cations, including the planar "aromatic" 10π electron $S_4N_4{}^{2+}$, and thus made an important contribution to the fascinating and rapidly growing field of sulfur nitrogen chemistry. We also used these superacid systems to prepare for the first time the ions Hg_3^{2+} and Hg_4^{2+}. Although the Hg_2^{2+} ion had been known for many years, it never seems to have been considered that, if two mercury atoms could be bonded together, chains of three or more mercury atoms might also be prepared. Indeed, in addition to the three and four atom chains, we soon made the compound $Hg_{3-\delta}SbF_6$ which contains the logical end member of this series, namely, an infinite chain of mercury atoms. This compound is a two-dimensional metallic conductor that has excited considerable interest among solid state physicists. Our work on HSO_3F and HF and on the solutions of SbF_5 and other fluorides in these solvents led in turn to more general studies on fluorine chemistry including the oxofluorides of bromine and iodine and the noble gas fluorides and oxofluorides. My interest in the latter compounds was particularly stimulated by the fact that I was able to predict the structure of many of these compounds by using the VSEPR model before their structures had been determined and in some cases before they had been prepared.

So the focus of my research in the last twenty years or so has been the preparation of new compounds and the study of their properties and structure. This is the aspect of chemistry that I find most fascinating. Of course there are many other important aspects of chemistry, but chemistry can only continue to advance and its applications continue to expand if we continue the search for new compounds. There is no end to this search, but every new compound that is prepared adds to our understanding of chemistry, adds to our understanding of the world around us, and has the possibility of having new and important applications that have not even been dreamed of. For me, the excitement of research is that it is an exploration into the unknown and one never knows what hidden treasure one will come across. One has to find one's own way. Nobody can tell you with certainty which way to go and following someone else may be a mistake.

One of the predictions that I made concerning the noble gas fluorides was that XeF_6 should have a nonoctahedral structure, despite the fact that the molecular orbital theoreticians had predicted a regular octahedral structure. That XeF_6 was indeed found to have a nonoctahedral structure gave the VSEPR model an enhanced reputation and popularity. My development of the VSEPR model is an example of the fact that I have always been interested in simple qualitative non-mathematical models—as I mentioned earlier, mathematics is not my strong point—and so at McMaster I came to particularly enjoy teaching the general chemistry course. But I came to realize that the conventional general chemistry course, as it has developed over the past 25 years or so since the advent of Chem Study, is much too oriented towards the solution of elementary physical chemistry problems and to the abstract ideas of thermodynamics and quantum mechanics. And it does not sufficiently emphasize the real basis of chemistry, which is properties and reactions, that is, organic and inorganic chemistry. This is not to say that the physical chemistry is not important, only that we need a more balanced type of introductory course. We need to attract more students into chemistry and to do so we need to show the students in the general chemistry course that one of the primary concerns of chemistry is making new substances and investigating their properties and reactions–that chemistry is not just a lot of theory to be learned but an activity in which imagination and creativity play an important role and a science of immense practical importance. There now seems to be increasing recognition that beginning students should be exposed to more reaction chemistry—so-called descriptive chemistry— both at high school and in the general chemistry course. There has been some movement towards the incorporation of this material into first-year text-books, but I became convinced that a new type of text-book was needed which made extensive use of reactions and properties (descriptive chemistry) from the beginning, because it does not make sense to simply add this material onto a conventional principles-oriented course. So I was finally persuaded to write a book of this type, with the invaluable aid of Peter Robinson and another of my former students, David Humphreys, together with Colin Baird from Western Ontario. This text-book, which was published in 1986, and is now appearing in its second edition, was one of the first of a new breed that employs full color to illustrate chemical reactions so as to bring some reality to the dull equations of the old-fashioned type of text-book.

After many years of teaching the general chemistry course, I have come to realize with some concern that learning to many students means being able to reproduce what is in the text-book or being able to work a set type of problem, but it does not necessarily mean understanding. Thus, once the course is over and the examination has been passed and the text-book sold, most students rapidly forget almost everything that they "learned" in the course. Somehow we must do more to ensure that students obtain a better understanding of what they are studying. I believe that, at high school and in general, chemistry students are overwhelmed by courses that have too much material and particularly too much abstract and theoretical material, and so they have no time to read widely or deeply in order to obtain the understanding that they should. There is a real danger that there will be a shortage of chemists and other scientists in the near future because too few students are now attracted into science courses. In my view we could go a long way to solving this problem by showing in our introductory courses that chemistry is an exciting, creative, relevant subject and not, as it so often appears to students, a very difficult, abstract, and mathematical subject. So often when people learn that I am a chemist, even though they have made great progress in their own careers, they say, "Oh, that was the most difficult subject that I had to do in high school or in college", and so rarely do they say that it was the most interesting or the most enjoyable subject that they had to take. We know that chemistry is not a difficult subject, certainly no more difficult than other usual subjects that a student has to take, and to us it is more than just interesting; yet the introductory course is so forbidding and so difficult for the average student that this is the memory that most nonchemists have of the course. My aim was to write a different, more easily understood, more interesting, and more relevant book, a book that was concerned with real chemistry and not just abstract principles and theories, a book that would attract students to chemistry and not repel them from it.

Sabbatical leaves and other study leaves have given me the opportunity to work in England, France, Germany, Switzerland, Australia, New Zealand, and India. So finally I was able to educate myself somewhat in the history, culture, and language of some foreign countries. The international nature of chemistry and indeed of all the sciences gives a scientist the opportunity to travel and make international contacts and friends and to learn foreign languages.

Apart from my interest in travel and the culture of other countries, I have managed to find time to participate in a number of sports. My early contact with the mountains in Wales led to a number of years of rock climbing and mountaineering in Britain, Scotland, and the Alps. This led to skiing, which my wife and I were able to participate in more actively when we came to Canada. A lack of any nearby mountains led to a decline in our climbing activities, but being on the shores of one of the Great Lakes led me to take up sailing a few years later. I first raced a dinghy on Lake Ontario and various nearby small lakes. It has been said, although there is little truth to it, that a willingness to crew in my dinghy was a primary qualification for joining my research group. Eventually a shortage of such students, or more importantly the fact that my younger daughter departed for university and was no longer available to crew, or perhaps it was just my advancing years, led me to take up exploring the Great Lakes with my wife in a cruising keel boat.

I was married in 1950 to Madge Garner, a school-teacher. We met on a mountaineering holiday in France and, as I have indicated, we have continued our mutual interest in outdoor pursuits ever since. We particularly like to return to France to visit our friends, to travel and to ski. We have two daughters who have also come to enjoy the same activities. Our younger daughter is a botanist, a graduate of Carleton University and the Unversity of California, and is currently doing research at the Smithsonian Institution. Our older daughter is a graduate of Waterloo and Carleton Universities in environmental studies and art history, and is working as an architectural conservationist with the planning department of the City of Hamilton.

JAMES FRASER MUSTARD

(October 16, 1927 -   )

Killam Laureate, 1987

James Fraser Mustard, born in Toronto in 1927, was the only child of Alan Alexander and Jean Ann (Oldham) Mustard. His father was in construction and his mother was a nurse who practised her profession until the early 1940's. Those who worked with her greatly admired her knowledge, skills, organization, and dedication; she undoubtedly passed her attitudes and abilities on to her son. Fraser (as he is known by all) attended Whitney public school and the University of Toronto Schools (UTS). Even at that time, when only boys were admitted, the competition to be accepted by UTS was strong; Fraser convinced his interviewer that he should be allowed to enroll by answering the question of how a beaver would build a dam — would it be curved downstream or upstream? Fraser Mustard has been an advocate of the problem-solving approach to education ever since.

UTS was a highly competitive school and Fraser spent his first two years there struggling near the bottom of a class of about forty very bright students. At that stage he had not learned how to work or study and, when he missed a subject in his first year in high school (as secondary school was called at that time), he was held back. Suddenly, he found most of the subject matter extremely easy and he moved into the top group in his class, where he remained. He recalls that the subjects that really interested him (science and mathematics) were "good fun", but that he had a "terrible problem" with languages and hated Latin. It was wartime, and Fraser managed to join the Artillery Corps in the Reserve Army at age thirteen since, because of his size, he could pass for the required sixteen; by the time he actually was sixteen, he was leading a gun crew. He was also the commandant of the Cadet Corps at UTS. His natural athletic abilities enabled him to star at football, and he also took part in basketball, hockey, and other sports. Upon graduation from high school he won the Nesbitt silver medal which is awarded by UTS to a student who has done the most for the school by example, discipline, interest in school activities, athletic achievement, and scholarship.

Fraser had originally planned to become an engineer because he thought that engineering would be based on solid scientific principles, but after spending a summer with civil engineers on an Ontario Hydro project in northern Ontario he realized that engineering was not what he had imagined and he decided that medicine would be more to his liking since he felt it would be worthwhile to "look after people", and he believed at the time that medicine had a logical base in knowledge of human biology. Later, he came to understand the limitations of clinical medicine and to appreciate the crucial importance of developing a basic understanding of not only the scientific aspects of medicine, but also of the social, economic, cultural, and genetic factors that influence health.

In 1946, places in medicine for students leaving secondary school were limited because of the large number of returning war veterans who were given precedence. Fraser was not among the students accepted that year, so he enrolled in Honour Science for his first year at the University of Toronto. He enjoyed the problem-solving approach to learning in Prof. John Satterly's first year physics course in which a set of tough problems was assigned to be done over each week-end and the examinations were "open book" so that logical thinking rather than memorization was essential.

After one year in Honour Science, Fraser was able to enter medicine and received his M.D. in 1953, winning the Cody Silver Medal, the Roy Simpson Scholarship in Pediatrics, and the Chapel prize in Clinical Medicine, despite his atrocious handwriting. On at least one occasion he was called in by a professor to decipher the answers he had put down on his examination paper. These years, however, were not entirely devoted to studying medicine. Fraser's interests were broad; he took honours history courses during his two premedical years and became president of the University of Toronto History Club. To ease the tension of concentrated study, he pounded on the piano at home. Fraser played tackle on the University of Toronto Intercollegiate Football Team which won the Championship in 1948. A team mate, Dr. John Evans, recalls that Fraser was "a fantastic tackle and the terror of the league"; thirty-five years later, Evans was still describing how impressed he had been by "Moose" Mustard, Canadian all-star tackle, exploding from the line of scrimmage as Varsity trampled the Western Mustangs. His football experiences had a strong influence on Fraser's vocabulary which is still larded with expressions such as someone's being "on side", "dropping the ball", being a "team player", or "tackling a problem and wrestling it to the ground". Many of the men who played university football in those days have achieved positions of power and influence and the football ties have often smoothed the path in some of Fraser's many activities other than that of being a research scientist. In addition to his medical studies and football, Fraser found time to court and, in 1952, to marry a classmate from Honour Science, Christine Elizabeth (Betty) Sifton.

After graduation from medicine, Fraser spent a year as a junior intern at Toronto General Hospital and came to the conclusion that there were very few fields of medicine in which he really wanted to work. He was attracted to hematology because he felt that it had become the most science-based subject in the practice of medicine. It was possible to obtain samples of blood readily for study, a great deal was already known about the red cell, and there were some effective treatments, such as administration of vitamin B12 for pernicious anemia, that could change the course of an illness. With these considerations in mind, Fraser went to see the Professor of Medicine at the University of Toronto at that time, Ray Farquharson, to discuss his future course since this was the point when he had to make a decision about specialization. It so happened that Sir Lionel Whitby, the

Regius Professor of Physics from Cambridge, was visiting Toronto at that time, and Prof. Farquharson introduced Fraser to him (probably after describing Fraser's outstanding abilities to the visitor). Whitby offered Fraser the opportunity to go to Cambridge on an Elmore Studentship in the Department of Medicine; in 1954, this studentship was worth about nine hundred pounds, a generous stipend at that time. In addition, the young Canadian was to be supplied with laboratory space and a technician. When he arrived at Cambridge in the summer of 1954 to begin his studies for his Ph.D. in Biology B, Whitby asked, "Now, what do you want to do?" Fraser drew up a proposal to investigate factors influencing leukemia, and at their next meeting was told that the experiments that he had outlined would require seven years to complete. Since Fraser's intention was to spend only two years at Cambridge before going back to Toronto to complete his clinical training for his Royal College examinations, it was evident that he would have to find a less ambitious project. His second proposal was to study the effects of storage of blood because there were many patients in whom thrombocytopenia was being produced by blood transfusions. Although Whitby did not consider this to be a great project, he knew it could be done in two years; so he approved it. Thus began Fraser's lifelong scientific interest in blood platelets and their role in hemostasis, thrombosis, and atherosclerosis.

The project at Cambridge required considerable organizing ability, and such an ability has become the hallmark of all Fraser's subsequent activities. The head of the Transfusion Service in East Anglia had to be brought "on side" so that blood could be obtained readily at the donor sites, and the staffs of the Addenbrooke and Papworth hospitals had to be persuaded to give the new researcher access to patients who were receiving transfusions. Eventually he was able to build a whole network of surgeons, hematologists, and blood transfusion experts to make it possible for him to do his work. Construction of such networks has characterized many of Fraser's endeavours, and his current position as President of the Canadian Institute for Advanced Research is entirely devoted to building networks among scientists, sociologists, economists, psychologists, and epidemiologists to investigate topics that require a cross-disciplinary approach.

Since there was no course work at Cambridge, Fraser had to find experts to consult in other fields besides hematology, such as biochemistry and statistics, to obtain the information he needed to complete his project and prepare a thesis. His thesis was finished after many rewritings since the first attempts did not satisfy Sir Lionel. Writing was a struggle; although the data were solid and the thoughts were clear and logically developed, stringing the written words together has never come easily. From his experimental work, he came to the conclusion that, during blood storage, some factor was being formed that caused a rapid fall in the platelet count when blood was transfused into patients and that, when blood was taken under conditions that minimized the activation of coagulation, this factor did not form. Based on our current understanding of platelet function, it is now apparent that this factor was probably a platelet aggregating agent. While Fraser was working at Cambridge, interest in thrombosis and its relation to atherosclerosis was developing in England because of the findings of Duguid, French, Crawford, and others, and that is when he came to see the relation between platelets (the major constituents of arterial thrombi) and arterial disease. Fraser also realized that very few investigators were studying the role of blood platelets in thrombus formation because at that time it was quite difficult to do so. When he began his investigations of platelets, thrombosis, and atherosclerosis, Fraser did not foresee that these fields would become as "fashionable" as they are now, and he comments drily that the key to acquiring an immense number of citations is to be in on the ground floor as he was.

The years at Cambridge changed the course of Fraser's career. Before he went he had intended to specialize in an aspect of internal medicine — hematology; when he came back, he was interested only in pursuing the path of research that he had begun. Upon his return to Toronto in 1956, Fraser spent a year at Sunnybrook Hospital as a senior intern and then became a Senior Research Associate there in the Department of Veteran's Affairs, with an appointment in the Department of Medicine at the University of Toronto. In 1958 he won the Royal College of Physicians of Canada Medal for his essay, "A study of the relationship between lipids, blood coagulation and atherosclerosis". Impressed as before with Fraser's abilities, Professor Farquharson offered him the "plum" of being senior resident at the Toronto General Hospital, and was nonplussed when the offer was refused — no one had ever refused this position before. Farquharson told Fraser that he was doing "totally the wrong thing" by determining to go on in a career in research, but wrote him a cheque for three thousand dollars (a large sum in those days) to supplement his meagre income. His generosity with money was matched by his generosity of spirit, for several years later he admitted to Fraser that the choice of a career in research had been the right choice.

From 1960 to 1963, Fraser was supported as a Senior Research Associate by the National Heart Foundation of Canada, which became the Canadian Heart Foundation. In 1963, while remaining a Fellow in the Department of Medicine, he became an Assistant Professor in the Department of Pathology, and was awarded a prestigious Research Associateship by the Medical Research Council of Canada. The choice of a career in research, however, involved much less in the way of financial rewards than what he could have earned as a practicing physician. He left Toronto in 1966 to become chairman of the Department of Pathology at the new medical school being formed at McMaster University in Hamilton.

These bare facts from his curriculum vitae give no hint of the exciting research projects in which he and his colleagues were engaged during the decade from 1956 to 1966, nor of the scrounging that he had to do in order to find funds to support his first seven years of research. The groundwork that was laid at that time led to findings that had a major influence on the development of our understanding of the role of platelets in the initiation of atherosclerotic lesions, in the growth of the atherosclerotic lesions, and in the thromboembolic complications of atherosclerosis such as heart attacks and strokes.

The laboratory at Sunnybrook was cramped and crowded, but the aging veterans who were patients in the hospital were excellent subjects for research since their diets and schedules were well controlled. Fraser also quickly established links with the Ontario Veterinary College at Guelph, where large pigs were available without cost from the Ontario Reformatory. Funds for research were minimal; so, to obtain high-fat dietary supplements for these pigs to induce the rapid development of atherosclerosis, Fraser enlisted the help of the chief dietician at Sunnybrook, who collected the butter that was returned to the kitchen on the patients' trays and gave it to him to take to Guelph.

Among those working closely with Fraser during these years were the veterinarian at Guelph, Dr. Harry Rowsell, who later founded the Canadian Council on Animal Care; Dr. Edmund (Tony) Murphy who went to the Department of Medicine at Johns Hopkins University in 1960, but continued to collaborate with Fraser until 1968; greatly impressed by Tony's erudition, Fraser described him at the time as "a genius" with a "sophisticated knowledge of mathematics, statistics and experimental design, as well as music, art, literature, wine and good food"; and his chief technician, Brigitta Hegardt, a formidable Swedish valkyrie who ruled the laboratory with a firm hand. Dr. Marian Packham joined the group at Guelph in 1963, and later moved to Toronto; she has been associated with Fraser's research activities ever since; he acknowledges now that over the years she played a large part in "keeping him in science". Dr. Michael Glynn and Dr. Edward Nishizawa were also part of Fraser's group in the early and mid 1960's. In 1963, with Dr. R.L. MacMillan, Dr. Hugh Smythe, and Dr. Kenneth Brown, Fraser set up the Blood and Vascular Disease Research Unit, housed at 86 Queen's Park, now the site of the McLaughlin Planetarium. Money for the capital equipment for this venture was raised privately, largely through the efforts of Mr. Percy Gardiner and Mrs. Charles Gundy, both of whom had to persuade the administrators at the University of Toronto to accept their own donations and their money-raising efforts on behalf of the new Unit. Foundations such as the Atkinson Charitable Foundation also provided some support. The spacious high-ceilinged rooms of the old house, which had been the residence of the President of the University of Toronto, were turned into laboratories, with quarters in the garage and basement for a few experimental animals. Very soon, every room was bulging with equipment and crowded with technicians.

Throughout the ten year period that ended in 1966, Thursday was research day at Guelph. Each Thursday, equipment, supplies, technicians, and investigators were packed into Fraser's big station wagon and transported to the Research Station of the Ontario Veterinary College where work could be done with the large pigs, with rabbits, and with the unique colony of dogs with various congenital deficiencies of blood coagulation factors, for example, hemophilia. This colony was developed by Dr. Rowsell and maintained with grants from the U.S. National Institutes of Health. These dogs proved to be invaluable for the extensive experiments that the group carried out on the mechanisms involved in hemostasis — the process by which a mass of aggregated platelets, stabilized by fibrin formed by the reactions of blood coagulation, can seal a small severed blood vessel. The results they obtained reinforced the concept that the same mechanisms are involved in the formation of arterial thrombi as are involved in the formation of hemostatic plugs. Thus it became evident very early why any therapy that interferes with the formation of arterial thrombi carries with it the risk of bleeding.

These research days at Guelph were long and arduous and woe betide anyone who left to have a normal lunch or was not prepared to stay until the last test had been completed. It was often well into the evening before the group was driven back to Toronto and its members distributed to their homes. Fraser's energy level was, and has remained, awesome. He has always left others panting in his wake — and picking up the details. After a full day that began at 7:30 in his lab or office, he thought nothing of working until 1:00 or 2:00 a.m., night after night, to review results, prepare grant applications, read and discuss the literature, draft papers for presentation and publication, and plan ways to influence the availability of funds for research in Canada. None of his colleagues could match this pace, and without conscious planning, they found they were taking turns in participating in this myriad of activities in the evenings.

The main thrust of Fraser's research during this period was directed toward trying to obtain evidence that platelets, as well as lipids, were involved in the development of atherosclerotic lesions. In attempts to find differences in platelets from subjects who had had clinical complications of atherosclerosis and control subjects, Fraser and his colleagues examined a number of methods of studying blood coagulation and platelets. Eventually they focussed on the measurement of platelet survival and turnover and demonstrated that platelet survival was shorter and turnover was more rapid in subjects with clinical complications of atherosclerosis. These differences were also evident when subjects were separated on the basis of their family history of the complications of vascular disease. Although there were statistically significant differences between the groups of patients, the ranges of the platelet survival times overlapped and therefore precluded the test from identifying individuals at risk. The studies did indicate, however, an association between accelerated platelet consumption and clinical evidence of vascular disease.

Once the method for measuring platelet survival was in place, the group used this technique to examine the effects on platelets of a variety of factors, such as smoking, diet, and drugs, in human subjects at Sunnybrook or in the experimental animals at Guelph. An indication of how far ahead of the field these studies were in the early 1960's is the fact that it was not until the late 1970's that other investigators became interested in the information that could be gained from measurements of platelet survival and confirmed the findings of "Mustard's group" about the effects of smoking and dietary lipids. The data showing that smoking reduces platelet survival provide some of the strongest evidence available that smoking (which was not acknowledged as a risk factor for heart disease until 1964 in the U.S. Surgeon General's Report) may alter or injure the walls of arteries and thereby affect platelet consumption. Diets rich in dairy products and saturated fats shortened platelet survival, whereas low-fat diets or diets rich in vegetable fat did not.

Fraser recalls that one of his most exciting research experiences involved observations about the effect of blood flow patterns on the deposition of platelets and other blood constituents on blood vessel walls. He had persuaded Ontario Hydro to build plastic models of vessel branches and bifurcations for him. These "flow chambers" were attached to the arterial circulation of the large pigs at Guelph, and blood was allowed to flow through them and returned through a vein — that is, an extracorporeal circulation was set up. The pattern of the deposits in these chambers mimicked the pattern in which atherosclerotic lesions occurred in the pigs and, when the flow chambers were disassembled, the deposits were found to be made up largely of platelets. The paper describing this fundamental observation, which Fraser believes is important in explaining the location of the sites where atherosclerosis characteristically develops, was rejected by the prestigious Journal of Clinical Investigation in 1961 because "no one has ever seen a platelet in an atherosclerotic plaque". The fallacy of this judgment has been amply demonstrated by subsequent findings in many laboratories, but, at the time, Fraser's group was bitterly disappointed by the rejection. Fraser's studies of platelets and his conviction that they played a role in atherosclerosis and its clinical complications were totally unfashionable in North America because the cholesterol theory of the origin of atherosclerotic lesions dominated the field and most cardiologists did not think thrombosis was important in causing the clinical complications. Now, the pattern of platelet accumulation and alterations of the endothelium around vessel orifices and branches has been examined in a variety of experimental animals by the more sophisticated method of scanning electron microscopy, which was not available when the original experiments were done. The results from the flow chamber experiments were eventually published in the Canadian Medical Association Journal in 1962.

In collaboration with Dr. Henry Movat in Toronto, Fraser's group was the first to demonstrate that platelets release factors that increase vessel permeability and behave like other inflammatory cells in their ability to phagocytose particulate material. These observations convinced Fraser that platelets played a part in injury of the vessel wall, and Dr. Packham was able to localize the sites of increased vessel permeability in the aorta of pigs and rabbits by administering the dye, Evans blue; this technique was in use by Movat's group to demonstrate increased permeability in the microcirculation of animals given subcutaneous injection of materials released from platelets. Again, the pattern of Evans blue accumulation in the aorta was similar to the pattern of atherosclerotic lesions and platelet deposition. A Norwegian scientist, Dr. Leif Jorgensen, who joined the group for a year in the mid-sixties and still continues to collaborate, did the detailed microscopic investigation of the deposits and of the vessel wall changes at the sites of increased permeability. The use of Evans blue to demonstrate increased vessel permeability has become the definitive method and has been adopted by many other investigators.

Other extensive work in this period was concerned with the group's increasing understanding of the mechanism of thrombus formation. They had a strong suspicion that the predominant form of arterial thrombus would not be occlusive, but would be a mural thrombus which would fragment and embolize into the distal circulation. A series of studies was therefore initiated to examine the effects of transient platelet aggregates on organ function. Working with the large pigs, the group established for the first time that, when transient platelet aggregates were induced to form in the blood vessels supplying the heart, these aggregates blocked the small vessels downstream and caused arrhythmia, focal areas of infarction, and sudden death. In areas where platelet aggregates lodged in the microcirculation, the wall of the artery was injured. Similar effects were observed in the renal circulation; chronic renal disease and high blood pressure were produced in these experiments. These studies too were far ahead of their time and the concept that emboli from mural thrombi contribute to sudden, fatal heart attacks was only slowly accepted.

The studies with drugs, however, have had the greatest influence on the course of subsequent developments. As is often the case in science, an unexpected result opened up a large new area of research. Some of the veterans at Sunnybrook Hospital were being treated for gout with the drug sulfinpyrazone, a uricosuric agent that has weak anti-inflammatory properties. Since there was no known reason to exclude them from the platelet survival studies, they were included, but when the data were analysed, Fraser and Tony Murphy realized that the patients who were receiving the drug did not exhibit shortened platelet survival as did the other atherosclerotic patients in the study. This serendipitous observation led to the finding that non-steroidal anti-inflammatory drugs such as phenylbutazone and aspirin inhibit platelet aggregation in response to some aggregating agents – most notably to collagen, the constituent at the end of cut blood vessels to which platelets adhere. Dr. Packham presented these results with sulfinpyrazone and phenylbutazone at a Canadian Society of Clinical Investigation meeting in January of 1966, and included the comment that aspirin had the same inhibitory effects. Further work on the effects of aspirin was carried out by Dr. Geoffrey Evans who presented it at a meeting in June of 1967 in the United States, with an abstract of the presentation published in Blood. This meeting was attended by many investigators who had become interested in platelets, and, in September, a letter to Lancet (which is a route for rapid publication) submitted by investigators in New York, "scooped" the definitive paper that the Mustard group was preparing about the inhibitory effects of aspirin on platelets; their paper appeared in 1968 in the Journal of Experimental Medicine. Rarely in the literature has credit been given to the Mustard group for their contributions to the findings about aspirin that changed the course of platelet research. This unpleasant experience left a lingering bitter taste, and led to the resolve by Fraser's colleagues that important new findings must not be divulged before a manuscript on the subject has been accepted for publication.

Although the biochemical reaction that these drugs inhibit (the formation of the aggregating agent thromboxane A_2) remained to be worked out by other investigators, clinical trials were launched with aspirin and sulfinpyrazone in patients who had had a myocardial infarct and patients who were having transient attacks of cerebral ischemia (little strokes). Fraser played a major role in the initiation and planning of the earliest trials, working closely with Dr. Henry Barnett, now Scientific Director of the John P. Robarts Research Institute. In retrospect, Fraser comments that these trials were begun before enough knowledge was available about the process of platelet aggregation and the mechanisms involved in thrombus formation. Subsequent studies, some of them by his group at McMaster in collaboration with Dr. Packham in Toronto, have made it clear that other aggregating agents (ADP and thrombin) to which platelets may be exposed, are practically unaffected by these drugs. Despite the fact that the overall benefit of chronic administration of aspirin on reinfarction, stroke, and death is probably less than twenty percent, aspirin is in wide use in patients judged to be at risk.

Although he played a key role in demonstrating their effects on platelets, Fraser is frustrated to see how the relatively small gains produced by these anti-inflammatory drugs, and others such as dipyridamole, have been exploited in clinical medicine by pharmaceutical firms. In his presentation at the recent J. Allyn Taylor award ceremony at the Robarts Institute, Fraser emphasized that, in his opinion, there are far more fundamental issues, and the administration of drugs is not going to have a substantial effect in preventing the thromboembolic complications of atherosclerosis in any except small, carefully selected subsets of patients such as those with unstable angina. Fraser is dismayed by what he considers to be the misdirection of huge resources in large-scale clinical trials involving "antiplatelet" drugs, and deplores the way scientific findings can be distorted by marketing pressures.

The dogs with hemophilia were also used for studies of the effects of nonsteroidal anti-inflammatory drugs on hemostatic plug formation and bleeding. It had been known for some time that these drugs exacerbated bleeding in hemophiliacs. With Dr. Torstein Hovig from Norway, and Dr. Jean Dodds, who often tended the dogs all night after they had been used in experiments, the group demonstrated that the combined effect of these drugs and the inability of the affected dogs to produce fibrin, led to the formation of very unstable hemostatic plugs. These were the first direct microscopic observations, documented on movie film, of the formation and breakdown of hemostatic plugs in hemophilia. Concurrently, with Dr. Jack Hirsh, fibrinolytic agents were used to illustrate in another way the importance of fibrin in the stabilization of hemostatic plugs.

Given the scope and magnitude of the work during this period, and the large number of original observations, it is not surprising that in 1965 the Royal College of Physicians of Canada took the unusual step of making Fraser a Fellow, although he had never taken time to attempt the formal examinations required of all specialists. Then, in 1967, this amazingly productive period of research culminated in the choice of Fraser Mustard as one of the winners of a Gairdner Foundation International Award.

The year 1966 brought major changes. The group at Guelph broke up, and the Blood and Vascular Disease Research Unit was dismantled. Refusing a large NIH grant and an attractive offer to take a nucleus of his group to Albany Medical College in New York state, Fraser instead chose to move to the new medical school being formed at McMaster University in Hamilton, with his former team mate, Dr. John Evans, as Dean. This decision was influenced by the success of the Gundy Report in the mid 1960's. Medical research in Canada had been very inadequately funded, and early in 1965 a group headed by Dr. Jack Laidlaw (and including Fraser) enlisted the help of their mutual friend and patient, Mr. Charles Gundy, Chairman of the Board of the Hospital for Sick Children in Toronto, to make a coordinated effort to develop an estimate of the overall situation. A general survey of the national needs of medical research in Canada was taken by Woods, Gordon & Co., management consultants; the scientists on the original committee, and Mr. Gundy, personally contributed to the cost of the "Gundy Report" that was prepared. This report was endorsed by more than six-hundred medical scientists and the deans of most medical schools. It was favourably received by the two cabinet ministers to whom it was presented and Fraser was interviewed about it on the national CBC news. The Federal Government substantially increased the funding base for medical research over the next three years, and this improvement in support helped to convince Fraser that he could stay in Canada to carry on his research.

Fraser's first administrative position at McMaster was as chairman of the Department of Pathology, and he had a major influence on the modernistic design of the new health sciences building and on the development of an undergraduate medical educational program that was unique in Canada, but has subsequently influenced the approach of many other medical schools. Two excerpts from the outline of the objectives for the new school, drawn up in 1967, illustrate the philosophy of how students were to learn: "emphasis on seeing the essence of problems and utilizing all the resources of the university, living or otherwise, to understand the problem and answer related questions" and "the student will collect much of his own data relevant to these problems and will form habits of study which can provide the basis for life-long self education". If these quotations have a familiar ring, it is because they characterize the approach that Fraser has taken in all his endeavours. Unfortunately, it is only exceptional individuals for whom this approach to learning is appropriate, and, despite an elaborate interview system, it is difficult to find enough talented individuals who thrive on self-directed learning; perhaps this is why McMaster was the first medical school in North America to have a freshman class with more than fifty percent female students! Rather than formal lectures to large groups, the McMaster approach involves small group tutorial sessions. This type of teaching is not easy for most educators, and Fraser had difficulty understanding why others could not inspire and excite the interest of their students in the same way that he could. Perhaps one of his greatest failings is an inability to understand that others, no matter how dedicated, do not have his flair for leadership or for recognizing and pointing the way to the core of a problem.

Fraser re-established his research laboratory at McMaster, where it remains, continued his collaboration with Marian Packham in Toronto, who transferred to the Department of Biochemistry there, and attracted a series of visiting scientists and post-graduate medical students, some of whom completed Ph.D. degrees under his supervision. It is perhaps a reflection on the lack of emphasis on research in North American medical schools, and on the lack of adequate remuneration for research in comparison to what can be made in practice, that nearly all of these individuals were recruited from overseas — Australia, Norway, France, Germany, Italy, Britain, Romania, Czechoslovakia, Poland, Nigeria, Japan. Foremost among them is Dr. Raelene Kinlough-Rathbone, a physician who came from Australia in 1967 as a Ph.D. student and has remained at McMaster, rising to the rank of Professor and becoming the co-investigator on Fraser's Medical Research Council research grant. It is she who supervises the running of the laboratory, co-ordinates the work, and oversees the preparation of material for publication, as well as carrying on her own research activities.

Most of the overseas contacts were made initially at international scientific meetings that Fraser attended, where his presentations whetted the young investigators' interest in studying in his laboratory. Most of this loyal group is now scattered all over the world, but they see each other at scientific meetings and renew the bonds between them that were forged by working with Fraser. But it was not all work. Close ties were also established with the Mustard family who generously entertained lonely visitors from foreign countries at their farm, Scamperdale, to which they retreated each Saturday night. There, the Mustards and their six children, born between 1954 and 1963, shared a farmhouse with another family with six children — Fraser considered that the atmosphere was relaxing! Visitors were always put to work building fences, chopping wood, constructing additions, painting, and carrying out other chores, but those who could (and some who barely could) downhill ski were taken to Beaver Valley on Sunday mornings. On the ski hill, Fraser appears almost as formidable as he did on a football field, and there is no mistaking the large, imposing figure hurtling down the slopes exuding power. Fraser learned to ski after he was forty and, since he was far too impatient to take formal lessons, he picked up what he could by skiing with the Packhams. His skiing technique is unorthodox, but very characteristic.

Fraser freely admits that his administrative tasks, and service on university and government committees and on Royal Commissions, took him away from work at the bench, and that his science has survived by "successfully exploiting the talents of all my good colleagues, and living off their accomplishments". In answer to the question, "What were the inspirational influences in your life?", Fraser unhesitatingly answers, "my colleagues". It is true that Fraser's scientific reputation could not have continued to grow without the contributions of others, including not only those who came from afar to study and work with him, but also the technical staff in his laboratory, and scientists in Toronto and McMaster with whom he collaborated. Nevertheless, it was his leadership that set the goals and ensured the continuing success of "Mustard's group".

Until quite recently, Fraser held court at Saturday morning meetings, attended by all those who were associated in any way with his laboratory, including Dr. Packham's group from Toronto. Current results were presented and discussed in detail, with incisive criticisms from Fraser who often exhibited a more in-depth recall of experimental minutiae than the graduate student or colleague who had done the "hands-on" work; new observations from the most recent scientific meeting that Fraser had attended were summarized and examined for their application to projects being pursued in the laboratory; and far-out new theories or hypotheses, usually generated by Fraser, were thrashed over and ways to test them were devised. Fraser has an enormous appetite for new data and expected his students and collaborators to have tested every possibility that he (Fraser) could conceive of. On one occasion, Fraser was digging for more and more information, but everything he could think of asking for was being duly produced. Finally, in exasperation that he could not find any avenue that had not been thoroughly explored, he asked, "Well, what don't we have? That's what I want". Needless to say he has been frequently teased about this incident. Fraser has a knack for turning a disappointing result into the basis for a new theory, however outlandish, that stimulates his colleagues to think about the topic under consideration from a completely different viewpoint. This feat of "standing on his head" to look at data was often performed at Saturday morning meetings, but might also be performed at the end of a tiring day with Fraser stretched out full length on the floor. In retrospect, it is impossible to convey the inspiration, stimulation, and excitement experienced by all those who took part in those Saturday morning meetings that Fraser led.

The evenings that were devoted to studying and composing manuscripts continued, often in Fraser's home where his wife fed delicious dinners to his students and colleagues before they began the hours of work that went on until after midnight. One cannot say, however, that Fraser was "writing" manuscripts since the usual format was for him to devour the extensive literature that his colleagues were expected to assemble on the topic under consideration and then spew forth ideas into a tape recorder. Fraser had the exasperating habit of saying, "Well, that takes care of that" when he was satisfied that the results were convincing, the arguments had been adequately formulated, and a worthwhile conclusion had been reached. He seems to be incapable of comprehending the amount of detailed work required to turn the transcript of the tape into a publishable paper complete with a materials and methods section, reference list, figures, summary, etc., all conforming to the particular style of the journal to which it is to be submitted. In the sixties and early seventies, these chores were often done by Marian Packham. More recently, most of the composing as well as the writing has been taken over by her and Raelene Kinlough-Rathbone, as Fraser's schedule has squeezed out the time that used to be devoted to this activity. One major review article by Mustard and Packham that was published in 1970 and became a Citation Classic was composed not only in Hamilton and Toronto, but also at the Mustard's farm on weekends, in various airports, and under every circumstance when a few spare moments could be found.

The experimental work at McMaster (and in Marian Packham's laboratory in Toronto with which Fraser remained associated) was a continuation of some of the projects that had been started earlier, but a number of new lines of investigation were also launched. It had become increasingly apparent that it would be useful to develop methods to isolate platelets from plasma in a way that would preserve their functional integrity. Considerable time was spent by Dr. Neville Ardlie before these techniques could be perfected, but they made it possible for the Mustard group, and other investigators, to study the response of platelets to various stimuli under a wide variety of experimental conditions. The experiments with platelets isolated from plasma have shown that there are several pathways through which platelet aggregation can be induced, that there are specific inhibitors for some of these pathways, and that there are only a few general inhibitors of platelet function. A wide range of inhibitors was studied in addition to the non-steroidal anti-inflammatory drugs — antibiotics, steroids, prostaglandins, reserpine, dipyridamole, thrombin inhibitors, local anesthetics, ticlopidine, and many others. The platelet "washing" technique has been the key to many of the subsequent developments in the Mustard and Packham laboratories.

Early in the 1970's, Fraser's habit of reading the research literature widely in fields other than his own led him to recognize the implications for stimulus-response coupling of the rapid turnover that inositol phospholipids undergo upon stimulation of cells; he also realized that platelets would be ideal for experimental work on these reactions. Again, he was away ahead of other investigators when he initiated experiments on the pathways of phosphatidyl inositol metabolism;

these studies were carried out by Dr. Nishizawa and Dr. Jaya Haldar, and then by Ph.D. students (Dr. John Lloyd and Dr. Lin Leung); more recently, his colleague, Dr. John Vickers, with Dr. Kinlough-Rathbone's help, has been immersed in this project. Now, the crucial importance in cellular responses of the second messengers formed from phosphatidyl inositols when cells are stimulated has been recognized by all, and there has been an enormous proliferation of investigations all over the world on this topic.

In a continuation of earlier work, Dr. J. Heinz Joist studied the effects of diet-induced hyperlipidemia in experimental animals, phospholipid transfer between plasma and platelets, and the effects of lysolecithin on platelets.

Dr. Hans-Joachim Reimers developed a method of recovering platelets after they had been induced to release all of their granule contents, and these "degranulated" platelets have been used extensively in studies of the mechanisms of platelet reactions. The surprising experimental evidence showed that, contrary to expectations, the changes in platelets resulting from their degranulation did not affect their ability to survive in the circulation. Of course, Fraser then wanted to know, "What does?" Dr. John Greenberg in Dr. Packham's laboratory found that removal of sialic acid from platelet membrane glycoproteins with the enzyme neuraminidase shortens platelet survival, and he and Dr. Margaret Rand went on to demonstrate that proteolytic cleavage of platelet membrane glycoproteins also resulted in rapid removal of platelets from the circulation. These findings may provide the reason for the shortened survival of platelets in the circulation of patients with thromboembolic complications of atherosclerosis observed by Mustard and Murphy in the early 1960's, since under these conditions the platelets could be exposed to the proteolytic enzymes plasmin and leukocyte elastase. In support of this theory was the finding by Dr. Peter Winocour that epsilon-aminocaproic acid, an inhibitor of plasmin, reverses the shortened platelet survival produced by inserting indwelling aortic catheters into experimental animals. Work by Dr. Diana Somers, a graduate student with Dr. Kinlough-Rathbone, and by Dr. Winocour has also established that continuous vessel injury in experimental animals shortens platelet survival.

Since the first event in the formation of a thrombus is platelet adherence to the vessel wall, Fraser initiated the development of techniques to quantify platelet adherence to artificial surfaces and to injured vessel walls, and to define conditions and agents that would modify the extent of platelet adherence. These investigations were begun by Dr. Jean-Pierre Cazenave in Dr. Packham's laboratory and continued at McMaster by Dr. Cazenave and Dr. Kinlough-Rathbone. They showed that many of the inhibitors that prevented platelet aggregation had little or no effect on the adhesion of the initial layer of platelets to a surface and that, although aspirin inhibited collagen-induced platelet aggregation, it did not inhibit adhesion or the release of granule contents from the adherent platelets. These findings helped to explain why drugs such as aspirin have only modest inhibitory effects on thrombus formation and do not inhibit the vessel wall thickening that occurs in response to vessel injury; this thickening is due to the proliferation of smooth muscle cells under the influence of the platelet-derived growth factor (PDGF) that the platelets release. Fraser was delighted when PDGF was discovered by Dr. Russell Ross and his colleagues in Seattle because it could help to explain the thickening of vessel walls around orifices and branches where Fraser's group had shown that platelets are deposited, and PDGF provided the explanation for the observations of his colleague, Dr. Sean Moore, that continuous vessel injury leads to the development of full-blown, lipid-rich atherosclerotic lesions in experimental animals, even when they are on low-fat diets. These findings have led to the gradual recognition by other investigators that continuous vessel wall injury, as well as high cholesterol levels, can lead to the formation of atherosclerotic lesions.

Following studies by Dr. Stefan Niewiarowski of the interaction of polymerizing fibrin with platelets, Fraser also pushed his colleagues Dr. Kinlough-Rathbone, and Dr. Elizabeth Harfenist in Dr. Packham's laboratory, to investigate the role of fibrinogen binding in platelet aggregation with the result that the Mustard group was the first to demonstrate that fibrinogen binds to platelets immediately upon their exposure to an aggregating agent and that it dissociates when the platelets deaggregate. Since then, other investigators have identified the fibrinogen receptor on the platelet surface, and fibrinogen, a bivalent molecule, is now established as essential in forming bridges between adjacent platelets. Characteristically, however, Fraser wanted to know whether fibrinogen really was essential for this purpose under all circumstances and set out to examine situations in which the accepted dogma that he had initiated might not apply. In collaboration with Dr. Marco Cattaneo who had access to patients with afibrinogenemia in Italy, the group established that platelets from these individuals aggregated normally in response to the strong aggregating agent, thrombin. Recently, Dr. Hidenori Suzuki, a visiting scientist from Japan, used an immunogold technique to show that when platelets are aggregated with thrombin there are large areas of contact between adjacent platelets where no fibrinogen is demonstrable. Once again, Fraser is "bucking the line" and it will be some time before other investigators accept the validity of the conclusion from these new findings that fibrinogen may not always have to be involved in binding platelets together.

Another Mustard scientific crusade has been his attempt to convince other investigators that human platelets in citrated platelet-rich plasma (the condition under which they are most frequently studied) do not behave as they would in vivo because the low concentration of ionized calcium affects their responses to aggregating agents. Presentations and publications about this subject have punctuated the years since 1972, but even Fraser's powers of persuasion have failed to convince more than a few other investigators that artefactual responses are generated by the use of citrate. Even fewer have modified their procedures because it is easy to obtain platelets for study in citrated platelet-rich plasma, and clinical laboratories use this method. Many researchers, when they begin to work with platelets do not take time to explore the literature or consult those who could guide them. Since Fraser naively expects others to delve into a subject in the same intense way that he would, he is continually frustrated by reading reports of experiments done with platelets in solutions with unphysiological concentrations of ionized calcium. When he receives articles for review in which experiments with platelets are described as being done in media without physiological concentrations of ionized calcium, the investigators are firmly told the error of their ways. At scientific meetings, unless he is a designated speaker, Fraser usually sits at the back of the room and makes no comments, but afterwards he "tackles" hapless speakers in the hallway, or at a poster they are presenting, to emphasize the potential fallacies of conclusions from experiments with platelets in inappropriate media.

By 1972, when he became Dean and later Vice President, Health Sciences, at McMaster, Fraser no longer had time to go to his laboratory each morning to direct (or redirect) operations and make sure the "team" was functioning effectively. However, Saturday morning meetings and evenings of work on research topics with his colleagues continued until well into the 1980's. Not only were the administrative duties at McMaster heavy, but Fraser was also taking active roles at various times on government councils and committees, many of which he chaired, and Royal Commissions. He was chairman of the Health Research Committee of the Ontario Council of Health from 1966 to 1973 and a member of it until 1977. An indication of the influence he exerted is given by the story that, when he was appointed to lead a Task Force on Health Planning in 1973, a Queen's Park observer was quoted in the Globe and Mail as having commented that "Fraser Mustard has become the ad hoc unofficial deputy minister of health planning". Fraser also served as a director of the Canadian and Ontario Heart Foundations, as president of the Association of Canadian Medical Colleges, as chairman of the Advisory Council on Occupational Health and Safety, as chairman of the Ontario Faculties of Medicine, as president of the American Society of Hematology, as president of the International Society on Thrombosis and Haemostasis, and as a member of the Board of Trustees of the Aga Khan University in Karachi, Pakistan. In this last role, he so impressed the Aga Khan that he dubbed Fraser "Canada's National Treasure". Fraser's concern about the importance of science-based innovation for Canada's future has resulted in his appointment to the Premier's Council on Science and Technology (Ontario), and to the Prime Minister's Council on Science and Technology.

Not surprisingly, Fraser has been offered the presidency of several universities, but his response has been the same as it was to Farquharson's offer of a senior residency — a polite but firm, "No thanks". Fraser has been invited to give many distinguished lectureships and to write major review articles; he has served on the editorial boards of a number of journals; taken part in peer review of countless grant applications and articles submitted for publication; attended scientific meetings all over the world. On these trips, he often rents a car and drives miles to explore the regions he visits; he has an amazing ability to find his way around and an alarming habit of letting the gas gauge go well below empty before he begins to try to find a gas station. If he has ever been in a city before, even European cities with their twisting streets that seem to change their names after each block, he has retained a mental map and can drive almost unerringly to his destination; he cannot explain the powers of observation and memory that make it possible to do this, but they must be related to his ability in science to see the broad picture while picking out the key details. He also walks for miles in a strange city to familiarize himself with its flavour, often with the excuse to his companions who are almost running to keep up with his long strides that he is looking for a suitable restaurant. Although the choice is usually excellent, it may be very close to the starting point of the journey. On at least two occasions, Fraser has absent-mindedly departed on a plane without wallet or identification. An indication of his resourcefulness and unflappability under these circumstances is that he has been able to change airports in New York City, strike up acquaintances with fellow passengers willing to lend him money, find someone he knows in the airport at his destination to help him out of his difficulties, and get through Customs and Immigration effortlessly.

Fraser's contributions in research, in education, and in public service have been recognized in many ways — The James F. Mitchell Award for Heart and Vascular Research, Elected Fellow of the Royal Society of Canada, Officer of the Order of Canada, the Canada Council's Izaak Walton Killam Memorial Prize in Medicine, the Robert P. Grant Award for contributions to progress by the International Society on Thrombosis and Haemostasis, honorary degrees from five major Canadian universities, and the J. Allyn Taylor International Prize in Medicine which he and Dr. Packham shared in 1988.

The years during which Fraser increasingly came to depend on his colleagues to keep the research efforts going were not without accomplishments. The list of papers published since 1956 on which Fraser is an author exceeds four hundred and thirty, but two hundred and thirty of these appeared between 1975 and 1988. Dr. Kinlough-Rathbone and Dr. Packham studied synergism between pairs of aggregating agents and ways of bringing about the deaggregation of platelets. Dr. Hallie Groves, a graduate student with Dr. Kinlough-Rathbone, carried out detailed investigations of the decline, with time, of the ability of an injured vessel wall to attract platelets to adhere to it, the effects of reinjury, and the modification by drugs of the response to vessel wall injury. Dr. Cazenave and Dr. Elisabetta Dejana examined the role of thrombin in the response of blood to vessel wall injury. Dr. John Vickers' work with the phosphatidyl inositols became more and more detailed as new techniques were introduced, and Dr. Winocour studied platelet hypersensitivity in diabetes and the effects of various lipid-containing diets on platelet function and survival in experimental animals. Dr. Chantal Lalau Keraly undertook extensive investigations of the effects of epinephrine (adrenaline) on platelets. Meanwhile, in Dr. Packham's laboratory in Toronto with which Fraser remained associated, Dr. John Greenberg and Dr. Margaret Rand studied the changes in platelet density that take place as platelets age; Dr. Harfenist continued her detailed investigations of the interactions between platelets and adhesive proteins; Maria Guccione and Dr. Stephen Lam studied platelet nucleoside diphosphokinase and inhibitors of cyclic AMP phosphodiesterase; and Dr. Avinoam Livne, a visiting scientist from Israel, did a major study of the association of phospholipids with the cytoskeleton of aggregating platelets.

One of Fraser's latest enthusiasms arose from his appointment in 1982 to the Medical Advisory Council of the Alcoholic Beverage Medical Research Foundation. He became fascinated by the epidemiological evidence that shows that a moderate consumption of alcohol is inversely related to the incidence of the clinical complications of coronary heart disease. As a result, Dr. Rand is currently studying the effects of alcohol on platelets, and Fraser has been regaling his audiences with advice to consume wine with meals, particularly meals that include fish, since fish oils contain a fatty acid, eicosapentaenoic acid (EPA), that has minor inhibitory effects on platelet function although its purported beneficial effects on the development of atherosclerosis may be exerted in other ways.

In 1982, Fraser founded and became president of the Canadian Institute for Advanced Research, based in Toronto, and he has gradually had to give up his own research because of the pressures not only of establishing the programs of the Institute, but also of raising funds for it from the private sector. During the last year he has seldom visited his laboratory or office at McMaster and keeps in touch only sporadically and superficially with the research activities that are still being carried on there under the direction of Dr. Kinlough-Rathbone with the help of Dr. Packham. Even Fraser's prodigious appetite for the literature in his field has been redirected to the interests of the Institute, and invitations to scientific meetings and to write review articles are reluctantly declined. Looking back, Fraser muses that one of his greatest frustrations is that he did not keep himself at the task of addresing the fundamental problem of the formation of atherosclerotic lesions at their characteristic sites and let himself be sidetracked by too many other issues. If he had time, he fantasizes, he would come back and "spend a lot of time" on this problem.

In typical fashion, Fraser has thrown himself whole heartedly into developing the Canadian Institute for Advanced Research (CIAR), which has taken a unique approach to the study of complex problems of scientific, social, and economic significance that are multidisciplinary in character. The Institute defines promising areas of research and creates networks of unusually talented individuals who remain in their own institutions but are supported by the Institute to enable them to concentrate on their research and to interact with others in the network. It had become clear to those who first conceived of the Institute that the nature of modern research is such that concentrations of highly qualified manpower and resources are required if Canada is to remain competitive on the world-wide scene. The universities are prevented from developing such resource bases since their function has become mass education with research as an appendage, and they cannot justify diverting their funds to respond to the needs of basic research in fields that are moving quickly.

The programs that have been developed by CIAR have taken Fraser into such diverse fields as Artificial Intelligence and Robotics; Cosmology; Evolutionary Biology; Superconductivity; Law and Society; Economic Growth, Technology and Change; and Population Health. Advisory Committees have been set up for each program by the Research Council of the Institute and over one hundred Fellows, Scholars, and Associates have been recruited. The originally Canadian character of the Institute is becoming more international as more and more individuals from the United States and Europe are being invited to join the programs and the Advisory Councils.

Fraser's endeavours on behalf of the Institute have changed and broadened his view of the future of science in Canada. He feels that the country has a limited time to awaken to the demands and pressures that all societies will face from the impact of science-based innovation on economic growth. Canada is still geared to a resource-based economy without a clear appreciation of the magnitude of the competitiveness that the future holds. Those who go into careers in science must recognize the significance of the merging of science and technology and the demands that will be placed on scientists and engineers if the prosperity of the country is to be sustained and enhanced.

Fraser now distinguishes several different categories of research. Developmental research has a short time horizon of two to three years and requires a strongly managed research structure within an industrial environment to be effective. Applied research, which can be considered as strategic research with a three to ten year time span, does have a focus on a specific product or service, but here, management has to be more flexible because serendipity, even at this stage, plays a large part in successful developments. The risks of not being able to develop something that will be commercially marketable are quite high, and Canada's capacity for this type of research is very weak. Finally, there is basic or long-term research which involves attempts to probe fundamental issues of intellectual breadth. This research needs an intellectual focus since it is directed at trying to answer some question perceived to be important, but the approaches and the nature of the results are extremely difficult to predict. In this kind of research, one needs a climate that supports creative innovation and provides for enormous flexibility of operation of the individuals. The expected result is significant advancement of the intellectual field, not the production of commercial products. It is important not to confuse the different levels of research, and it is essential that people going into the different fields understand the importance of the other fields to their own work. Creating effective synergism among the different levels of research is an extraordinarily difficult task, but has been done successfully within some organizations such as the pharmaceutical industries and the Bell Laboratories. It became apparent to Fraser that Canada has a fundamentally weak industrial capacity in research that is unable to interact effectively with basic research being carried out in universities. To respond to this deficiency, Fraser had led the CIAR to champion the development of a higher level of industrial competence to interface with basic research in areas of key generic technology by promoting the establishment of PRECARN. This organization is a non-profit consortium in which thirty-four corporations and government laboratories joined together to sponsor, manage, and disseminate the results of long-term "precompetitive advanced research" projects in the area of artificial intelligence systems and advanced robotics.

In Fraser's opinion, it is important for young people to understand that the prosperity of the West is of recent origin and has been achieved primarily through innovation, both in technologies and in the establishment of institutions. In this century, innovation has become increasingly science-based, and, for the remaining part of this century and for the next, science-based innovation will be the dominant factor behind economic growth. If Canada can grasp the opportunity to take part in the technology revolution, it will be able to sustain and enhance its prosperity, giving it the resources necessary for a strong basic research capability in the sciences. If Canada fails to grasp this opportunity, it will not have the resources to make the long-term investments that are required to sustain the quality of life to which we have become accustomed. Support for basic research will also suffer. At present, it is not clear which way Canada will go.

It will no longer be possible for people in the different sectors of research to remain isolated from each other. For a nation to have a successful innovation policy, it will have to create institutions that more effectively bridge the gap between fundamental research and developmental research in industry. Without an effective capacity in science-based innovation, Canada will be unable to sustain its current prosperity. A nation that cannot sustain its prosperity and goes into decline has enormously complicated problems, including a gradual degradation of the social environment. Thus it is important for Canada to develop a cadre of scientists and engineers for the future who are prepared to lead and apply themselves to the creative roles necessary for effective science-based innovation in a country that must be recognized as having a peripheral economy in the global system. This leadership will require individuals who are also prepared to deal with the financial institutions, corporate management, and government policies that directly or indirectly have an impact on science-based innovation.

ASHOK KUMAR VIJH, O.C.

(15 mars, 1938–   )

Prix Izaak Walton Killam, 1987

Juillet 1988. Sur la scène du Festival de Jazz de Montréal, Johnny Clegg, le zoulou blanc, danse et chante l'amour et la liberté avec ses amis noirs du groupe Savuka. Devant la Place des Arts, sur la rue les terrains, 60 000 personnes vivent au rythme des chansons du groupe sud-africain. Dans cette foule bigarrée qui fraternise, Ashok Vijh et sa compagne, Ratna Ghosh, savourent ses instants de magie.

Ce descendant de colon blanc et ces fils d'esclaves noirs que le ridicule de l'apartheid a contribué à rapprocher, unis par un air de concertina et le son du tambour, ont la libération joyeuse et communicative. Et bien que la scéne et la science soient deux mondes biens différents, ils ne sont pas si loin l'un de l'autre, le chanteur-musicien et le physicien-électrochimiste: chacun à sa manière a pris possession du monde, pour le mieux connaître, l'apprivoiser, le changer...

Ashok Kumar Vijh est né à Multan, dans la province indienne du Panjab. Suite à la partition des Indes britanniques en août 1947, la petite ville se retrouva en territoire pakistanais. La famille Vijh, à l'instar de 10 millions de réfugiés hindous, rejoignait l'Inde, tandis que 7 millions et demi de musulmans allaient prendre le chemin du Pakistan.

Multan était une petite ville au climat extrêmement sec, presqu'un désert. Sans grand tradition intellectuelle, elle ne possédait, à cette époque, ni université ni institution d'enseignement post-secondaire de quelque importance.

En dépit de cela, cette région a fourni deux des trois lauréats du Prix Nobel vivants nés dans le sous-continent indien: Har Gobind Khorana et Abdus Salam; le troisième, Subrahmanyan Chandrasekhar, d'origine Tamoul, est également né au Panjab.

La famille

Les Vijh étaient une famille de la classe moyenne. Bishamber Nath Vijh, le père, occupe un poste important dans la haute fonction publique. C'est un homme de grande culture qui parle aussi bien l'hindi, l'urdu, le sanskrit que l'anglais, et qui attache la plus grande importance à l'érudition et à l'excellence. Prem Lata Vijh, née Bahl, la mère de Ashok Kumar, est issue de la petite noblesse terrienne. C'est une femme courageuse qui a obtenu un succès spectaculaire en réalisant la tâche herculéenne d'élever une famille de dix enfants. Ashok Vijh est le second enfant de cette famille. Tous les frères et soeurs Vijh ont fait des études universitaires et sont professionnellement bien établis mais aucun, à l'exception de Ashok, ne s'est dirigé vers la recherche scientifique.

Tout juste après avoir obtenu son doctorat, en 1966, Ashok Vijh épouse une camarade étudiante de l'Université d'Ottawa. Malheureusement, ce mariage devait se terminer par un divorce en 1986. Cette union aura porté fruit, puisqu'un fils exceptionnel, Aldous Ian Vijh, est né le 23 octobre 1970. Il vient d'entreprendre des études collégiales qui devraient le mener à l'université, en sciences sociales.

L'actuelle épouse d'Ashok Vijh, Ratna Ghosh, est détentrice d'un doctorat et professeur titulaire à l'Université McGill. Professionnelle des sciences sociales, elle s'intéresse profondément à la littérature anglaise ainsi qu'à la musique et à l'art. Elle est présidente de l'institut indo-canadien Shastri, un consortium formé de 22 universités canadiennes et de la Bibliothèque nationale du Canada qui a pour but de fournir aux indologistes (également appelés indianistes) les infrastructures nécessaires à la poursuite d'études avancées au Canada.

Les études

Après avoir obtenu les grades de bachelier puis de maître en sciences (chimie), de l'Université du Panjab, avec la plus haute distinction, Ashok Vijh vint au Canada pour entreprendre des études doctorales en chimie physique. Son choix se porta sur le Canada pour deux raisons principales: la première tenait au fait que le Canada lui paraissait alors comme un heureux mélange de «vieux» monde et de «nouveau» défi: un pays riche du Commonwealth, dont les traditions britanniques sont familières aux Indiens, mais un pays qui a également la vigueur et la vitalité qui caractérisent l'Amérique du Nord (à cette époque, Ashok Vijh ne connaissait pas l'extraordinaire tradition française du Québec dans laquelle il vit, travaille, et s'est épanoui avec autant de succès).

La deuxième raison de sa venue au Canada était la réputation du professeur Brian Conway de l'Université d'Ottawa avec qui l'un de ses professeurs de l'Université du Panjab avait étudié à la fin des années 1950.

Avant d'entreprendre des études universitaires, Ashok Vijh n'était pas particulièrement intéressé aux sciences. Sa préférence allait plutôt à la littérature anglaise et à ce qui était alors convenu d'appeler les «humanités» . Or, orienter ses études en ce sens l'aurait contraint à subir la pression paternelle et à compétitionner en vue d'obtenir un poste dans l'administration gouvernementale indienne ou les affaires étrangères. En ne choisissant pas cette voie, mais la science, c'est à une vie de «fonctionnaire» qu'il a échappé.

Pour lui, l'important était de réaliser quelque chose d'original, de créatif, d'intellectuellement excitant. C'est ainsi que rejetant a priori l'administration publique ainsi que les carrières traditionnelles en médecine et en droit parce qu'elles n'offraient pas cette opportunité d'accomplissement qu'il recherchait et n'ayant aucune attirance particulière pour les affaires (en fait, de telles carrières lui paraissaient vulgaires et ennuyeuses), c'est à contrecœur qu'il se résout à la science. Ajoutons qu'en Inde, début des années 1960, les candidats à la carrière scientifique avaient l'opportunité d'obtenir des bourses d'études et de poursuivre des études avancées à l'étranger: en soi, ce n'était pas négligeable.

Ashok Vijh arrive donc au Canada le 23 février 1962 et obtient son doctorat de l'Université d'Ottawa en 1966. Le manque d'opportunité et l'absence de défis intéressants que comportent les offres d'emploi qui lui sont faites au Canada, l'amène à accepter un poste de chimiste principal aux laboratoires de recherche et développement de la compagnie Sprague Electric, du Massachussetts.

En 1969, le nouvel Institut de recherche d'Hydro-Québec était formé sur la Rive-Sud de Montréal. Le directeur-fondateur, Lionel Boulet, propose alors à Ashok Vijh le poste de chef de groupe, avec mission de mettre sur pied la recherche en électrochimie. Parallèlement, on lui offre un poste de professeur invité et de directeur de thèses à l'Institut national de recherche scientifique (IRNS-énergie) de l'Université du Québec, poste qu'il occupe de 1970 à 1978 et de 1981 à aujourd'hui.

À l'Institut de recherche d'Hydro-Québec, il est chef de groupe de 1969 à 1973, chef de groupe et maître de recherche en 1973–1974, puis chef de programme et maître de recherche de 1974 à 1981 et, enfin, maître de recherche depuis cette date.

La recherche

L'électrochimie moderne est un vaste domaine qui traite, de façon générale, des réactions chimiques impliquant un transfert d'électricité à travers des interfaces. Les contributions originales et innovatrices de Ashok Vijh servent de toile de fond à la compréhension de ces réactions. Ses travaux ont contribué à la connaissance des procédés qui entrent en jeu lors de la conversion et du stockage de l'énergie (piles à combustible, accumulateurs, dispositifs photoélectrochimiques, production d'hydrogène électrolytique) et sont utilisées dans l'industrie chimique (production de l'aluminium, du cuivre, du zinc, de la soude caustique, du chlore et du fluor).

L'impact international des recherches de Ashok Vijh fut immédiat. Sur le plan de la compréhension des réactions électrochimiques, ses découvertes furent considérées comme majeures. Il a découvert comment les propriétés solides des électrodes déterminent les cinétiques des réactions électrochimiques à travers des interfaces électrode-électrolyte. Par le rapprochement des concepts de la physique des solides et de l'électrochimie, ses travaux ont jeté une lumière nouvelle sur cette science et en ont élargi le champs d'application, contribuant à créer une nouvelle discipline scientifique appelée physique électrochimique.

De plus, au-delà de ses qualités de chercheur, Ashok Vijh a su créer à l'Institut de recherche d'Hydro-Québec un centre de recherche électrochimique mondialement reconnu.

La contribution la plus marquante d'Ashok Vijh a été son étude de l'interface semiconducteur-électrolyte, publiée en 1973 sous le titre *Electrochemistry of Metals and Semiconductors*. Cet ouvrage a été largement acclamé par les scientifiques du monde entier et lui a valu le Prix Lash-Miller de la Electrochemical Society en 1973. Les recherches de Ashok Vijh ont donné lieu à quelque 250 articles scientifiques dans des périodiques de recherche internationaux ainsi qu'à cinq volumes et plusieurs critiques de livres rédigées sur invitation.

Membre titulaire (Fellow) de l'Institut de Chimie du Canada et de la Royal Society of Chemistry, d'Angleterre, en 1973; de l'IEEE (Institute of Electrical and Electronic Engineers) des États-Unis, en 1982; de l'Institute of Physics d'Angleterre, en 1984; de la Société royale du Canada (Académie des sciences), en 1985; de l'American Physical Society, en 1986; fellow de la National Academy of Sciences de l'Inde, et associate fellow de la Third World Academy of Sciences, en 1987. Ashok Vijh reçoit le Prix Noranda de l'Institut de chimie du Canada en 1979, le Prix Urgel-Archambault de l'Association canadienne-française pour l'avancement des Sciences, en 1984, et devient chevalier de l'ordre du Québec, en 1987. La même année, il obtient le Prix Izaak Walton Killam du Conseil des Arts du Canada. En 1988, il se voit décerner le titre de docteur en Droit (Honoris Causa) de l'Université Concordia de Montréal. La Société royale du Canada lui remet la médaille Thomas W. Eadie en 1989, puis il devient Officier de l'Ordre du Canada et reçoit la médaille de l'Institut de chimie du Canada (CIC Medal) en 1990.

Bien que ses travaux soient directement reliés à la science, leurs impacts ont été souvent ressentis dans le domaine de l'ingénierie (génie chimique, génie physique et génie électrique). C'est la raison pour laquelle le Prix Killam lui a été décerné en ingénierie, même s'il n'est pas ingénieur.

Soulignons, enfin, qu'il est (ou fut) membre du comité éditorial de 13 périodiques scientifiques nationaux et internationaux en chimie, en physique, en sciences des matériaux, en énergie, en sciences des surfaces et en technologie.

La société

En plus de ses recherches en électrochimie et en physique électrochimique, Ashok Vijh est profondément concerné par l'interaction de la science avec la société telle qu'elle apparaît, entre autres, face aux problèmes de la prolifération nucléaire et du changement global. Il étudie et écrit sur la politique et la sociologie de la science, sur les bases intellectuelles de la créativité, sur les facteurs qui encouragent ou inhibent le processus de création des chercheurs.

Ashok Vijh n'aime pas qu'on aborde sa carrière en terme d'accomplissement. Il ne considère pas la gloire que la communauté nationale et internationale a fait rejaillir sur lui comme une finalité. Il aime dire qu'il se sent, encore aujourd'hui, comme ce jeune homme qu'il fut, avec son doctorat sous le bras, prêt à défier le monde entier. Heureux que ses pairs aient reconnu la valeur de ses travaux, bien sûr, mais surtout décidé à poursuivre aujourd'hui comme hier, cette soif intellectuelle qui l'a toujours poussé à s'attaquer aux problèmes qui l'intéressaient.

Dans de nombreux écrits et à travers plusieurs entrevues qu'il a accordées au cours des dernières années, Ashok Vijh a voulu prendre position sur la place de la recherche scientifique dans notre société. Pour lui, il est évident que la recherche fondamentale est l'un des investissements les plus importants que puisse faire un pays. Il s'agit du même type d'investissement que l'achat de semences en vue de récoltes futures, aime-t-il à répéter. Non pas que la société ne doive rien attendre de concret de la recherche, au contraire, mais, pour ce faire, elle ne doit pas lui donner d'objectifs à trop courte vue. En outre, la recherche de pointe ne peut être entièrement prévisible, sinon elle ne serait que triviale. C'est pourquoi, lorsque'elle aboutit à des résultats inattendus mais intéressants, même si ses buts premiers se sont quelque peu perdus en route, il faut considérer qu'elle valait la peine d'être réalisée.

L'excellence

Pour Ashok Vijh l'idéal pour une société est d'exceller à tous les niveaux: autant en recherche fondamental, en recherche appliquée, en développement, en démonstration, etc., mais on ne peut pas sauter les étapes. Il faut créer l'environnement intellectuel et établir les traditions et la culture interne qui permettront à ce processus de s'épanouir. Selon lui, l'un des plus grands dangers qui guettent la plupart des institutions est leur tendance à sombrer dans un certain laisser-aller qui se traduit par une démarche somnambulique vers la médiocrité. Il faut prendre garde que cela n'arrive pas à nos institutions, et contre cela la seule réponse est la promotion de l'excellence à tout prix.

Devant les coupures budgétaires qui affectent la recherche au Canada, il n'a trêve de condamner ces actes de mutilation criminellement imprévoyants de nos structures scientifiques. Il s'attaque avec énergie au laxisme gouvernemental qui a mené le Canada à n'allouer que 1,3% de son PNB à la recherche alors que la plupart des pays industrialisés y consacrent de 2,5 à 3,0%. Les «modes» en recherche ne trouvent pas chez Ashok Vijh un allié. Il condamne les organismes de recherche qui ont lamentablement cédé aux pressions politiques en éliminant des programmes de recherche de première classe au profit de recherches publicitaires de style «Mickey Mouse» reliées à la navette spatiale, etc. Il croit que nous devrions avoir des objectifs à long terme, des stratégies et des infrastructures pour mener le type de recherche productive qui mène à la prospérité. Tout pas de danse de fantaisie en accord avec la mode dernier cri ou toute pression suscitée par les politiciens ne mènent à toute fin pratique qu'au gaspillage des fonds publics.

Conséquemment, Ashok Vijh, se sent très concerné par le manque d'enthousiasme et de vigueur qu'on observe de plus en plus chez les scientifiques. Il faut que les chercheurs reprennent la destinée de la science en main. Que des leaders dynamiques sortent du rang, des scientifiques hautement motivés et convaincus de leur démarche, qui sauront protéger les chercheurs contre de stupides demandes du système, qui refléteront un modèle d'excellence à l'échelle mondiale.

{avec la collaboration de Régent Boucher (IREQ)}

H.J.M. BARNETT

(February 10, 1922–)

Killam Laureate, 1988

Henry Barnett was born in 1922 in Newcastle-Upon-Tyne, England. He was the third child of a Dublin Irish father and a Glasgow Scottish mother.

His father, a Church of England clergyman, had recently completed his theological studies at Durham University. A major and driving ambition of his parents was that each child should be given the opportunity for an excellent education. To achieve this, the family emigrated to Canada in 1925. Reverend, later Canon Barnett's career was then spent in the Diocese of Toronto, where six of his seven children received University educations. On the salary of a clergyman in industrial east Toronto, heavily hit by the Great Depression, this was no mean feat.

Henry Barnett had a foreshortened formal primary education. Birch Cliff Public School in Scarborough, east of Toronto's boundaries, was experiencing a crisis in accommodating the growing number of students in the area. By arrangement with the school, Barnett was taught to read and write by his mother and did not register in the public school system until the age of 7 $\frac{1}{2}$. Ten years later he graduated from The University of Toronto Schools (UTS) with the equivalent of present day grade 13. UTS stressed scholarship, and there can be no doubt that the excellence of the staff and the example of achievement-oriented classmates played an important role in his formative years. The teaching qualities of the staff were exemplified by Dr. J. G. Althouse; he was even able to make latin and ancient history classes lively and attractive.

The Royal Ontario Museum was three blocks removed from UTS. A chance encounter with a junior staff member in its zoology department, on a Saturday afternoon walk, introduced Barnett to the world of natural things. They were both out exploring the east Toronto lake-front remnant of one of the historically magnificent waterfowl marshes in North America. For Barnett, this was only the beginning of a life-long association with James L. Baillie Jr. and the world of nature which had previously been unnoticed by him. Baillie, a high-school drop-out, who eventually became the Curator of Ornithology at the Royal Ontario Museum, and J. Murray Spiers, a Ph. D. student in ornithology, had a considerable impact on Barnett between the ages of 12 and 16. They emphasized the importance of careful observation and scrupulous honesty in reporting scientific material, even though it referred only to the sighting and reporting of birds. Mr. A. Crowell, head of Biology at UTS, encouraged Barnett's interest in nature and promoted a career choice in the natural sciences or biology. But Barnett's practical-minded father considered biology frivolous and declared that he could not afford to help fund a university education in biology. He was prepared, however, to make the sacrifices needed to contribute to a career in something that would "lead somewhere". Medicine was the compromise solution.

Barnett entered medical school without a burning enthusiasm for physics, chemistry, or mathematics, but with an avid interest in descriptive biology. He grew up without much athletic prowess, and with little knowledge of music or art. He did not share his family's enthusiasm for commitment to a formal religious institution.

In 1942, all students in Canadian medical schools were inducted into the Royal Canadian Army Medical Corps so that they would not abandon medical training to join the armed forces. Summer vacations were shortened to speed the completion of their training. During his last two short medical school summers, Barnett worked in the Department of Pathology at the Banting Institute under the direction and stimulus of Professor William Boyd. This master of pathology was the author of the standard textbook of pathology of the time, used in most medical schools throughout the world. An extremely able teacher, he encouraged Barnett to consider academic medicine as a career, and, more than any other of his previous or later teachers, taught him the importance of making a scientific presentation as clear and as interesting as possible. Barnett's summer job involved the preparation of specimens and case summaries for the teaching material in the Museum of Pathology which Boyd had developed over a 25 year period. For medical students of that era, the Museum was one of the outstanding features of the undergraduate training program. Pathology in those days was one of the most advanced of the basic medical sciences in Toronto.

Two months after turning 22, Barnett completed his medical education and received an MD from the University of Toronto. The 1944 graduation was followed by an 8 month junior internship foreshortened by four months from the traditional year to facilitate transfer to active duty in the Royal Canadian Army Medical Corps. Active military service for most of the 1944 graduates was abbreviated and of little consequence because the War concluded in Europe in May of 1945 and in the Pacific in August.

The tedium, confusion, and mindless bureaucracy of the postwar military service came to a happy ending in July, 1946, when William Boyd offered Barnett a Fellowship year in Pathology at the Banting Institute. Here he came under the additional influence of Professor Eric Linell, Professor of Neuropathology. Post graduate training in Internal Medicine for two years at the Toronto General Hospital (TGH) and Sunnybrook Hospital (SBH) was succeeded by a year devoted to clinical neurology under Dr. H. H. Hyland in TGH, and then by a year in London, England, Queen Square, the traditional training-ground for clinical neurologists of the time. Sir Charles Symonds, one of the last of the British masters of descriptive neurology, and J. G. Greenfield, one of the world's leading neuropathologists, were important influences in this British study period. They demanded exactitude in clinical study and in the relationship of the signs and symptoms of disease to pathology. They became personal friends in addition to being stimulating mentors.

After Queen Square, Barnett spent a few months at Oxford as a clinical assistant to the Chief of Neurology, W. Ritchie Russell, and the Chief of Neurosurgery, Sir Hugh Cairns.

As important to Barnett as these mentors were his peers throughout the training program. His first hospital breakfast, as he began internship, was shared with a University of Western Ontario graduate, Dr. Charles G. Drake. A lifetime friendship developed with Drake, who was pursuing neurosurgery *pari passu* with Barnett's study of neurology. Eventually they married two young nurses, who were already good friends. They rented a house together during the British sojourn. Over the years ahead, two things emerged from this association. First was the creation in 1969 of a Department of Clinical Neurological Sciences at the University of Western Ontario, of which Drake was the Chairman of the Division of Neurosurgery, Barnett of the Division of Neurology; the Departmental Chairmanship rotated between them on a 5-year basis. The determination of both was that the clinical science of neurology, using the term generically, would be strengthened by cohesive collaboration between the clinical scientist-physician and the clinical scientist-surgeon. Most of the problems demanding solution were and remain of common interest and their pursuit would be enhanced by joining these traditionally separate interests and by linking both to neuroradiology and neuropathology. A second but later bond between Drake and Barnett was the marriage of a Drake son and a Barnett daughter producing common grandchildren.

Barnett cannot claim a background of basic science nor of laboratory methodology. His early ambition was to become as close to being a master of bedside and clinic neurology as was within the limits of his talents. Those who influenced him had instilled a drive to use meticulous observation followed by a synthesis of all available evidence to allow as accurate a localization and etiological diagnosis as the clinical methods of the time permitted. This was not an innovative nor ground-breaking approach, but it led to the development of a very large experience in neurological illness. In 1952, he was appointed as Assistant Professor in the Department of Medicine at the University of Toronto and to the staff of the Toronto General Hospital. There was not a separate department of neurology nor of neuroscience. He was the first Toronto neurologist to practice neurology without combining it with psychiatry. From the sheer volume of his clinical activity, applying the rigorous methodology of bedside observation, coupled with the radiological imaging of the brain and its blood supply by the relatively primitive methods of the 1950's and 1960's, he and others were able to shed some new light on arterial and cardiac mechanisms causing stroke and stroke-threatening symptoms. These studies set the stage for the next two decades which were to be spent in evaluating strategies to prevent stroke.

But he did not confine his special interests entirely to stroke and cerebral vascular disease. Prior to the introduction of antibiotics, a spinal cord injury of sufficient severity to cause paraplegia led to death within weeks or months, because of the ravages of infected bedsores and urinary tracts. After World War II this type of spinal injury was no longer fatal, and paraplegic units were developed to train the victims to live normal lives even though confined to wheelchairs. Dr. A. T. Jousse pioneered this new program in Canada at the Lyndhurst Lodge Hospital, Toronto. Late in the 50's and early 60's, Jousse began to share perplexing neurological problems besetting some of these patients with his friend Barnett. In due course they identifed a new entity, post-traumatic syringomyelia. Syringomyelia itself had been described a century earlier; it consisted of a fluid-filled cavity occupying and replacing, by expansion, the central portion of the spinal cord. The appearance, the detailed description from clinical observation, from radiological investigation, and in time from pathological examination of these post-traumatic patients and their spinal cords opened the door to a new understanding of this uncommon disorder. The only monograph on this subject in the English language (the only other one appeared in Germany before the turn of the century) was published by Barnett and two colleagues. It re-classified the disorder, shed some new light on its causes, and remains the standard reference on the subject. Barnett regards himself as wedded to the pursuit of stroke problems but finds that he has a persistent attraction to his mistress: syringomyelia and its management. Anton Chekhov, the Russian playwright, a physician, delared once that he was married to medicine but that his mistress was literature. From time to time, he said, he deserted domesticity and dallied awhile with his mistress.

A major turning-point in Barnett's career occurred in 1969 following the acceptance, by Dean Douglas Bocking and Vice-President Harold Warwick at the University of Western Ontario, of the concept of a union of neurology and neursosurgery in the single new Department alluded to above. Professors Ramsay Gunton and Angus McLachlan were prepared to relinquish the direction of neurology and neurosurgery from their traditional Canadian places under the Professors of Medicine and Surgery respectively. Fifteen years later London's clinical neurosciences, represented in 1969 by only 3 neurologists, 3 neurosurgeons, 1 neuroradiologist and no neuropathologist, emerged as a very active and successful Department of Clinical Neurosciences with 24 neurologists, 7 neurosurgeons, 4 neuroradiologists, and 4 neuropathologists. These interdisciplinary appointees proved that they could work together in productive academic harmony. Progress in the detailed study of and research in clinical problems was facilitated by the focus recommended by Barnett and Drake and by the agreement accepted by the Departmental members that there would be specialization within the Department with particular emphasis on: cerebral vascular problems (understandable, with Drake already established as the world's prime expert in surgical management of cerebral aneurysms); multiple sclerosis; epilepsy; and neuromuscular disorders. A critical mass of expertise in these areas developed and within a decade the productivity and excellence of clinical neuroscience in London was known throughout the world with a reputation it continues to enjoy. Two chairs in the Department were endowed. One is held by the Department Chairman and named in honor of two distinguished London philanthropists who were major benefactors of the Department: Richard and Beryl Ivey. The other is named in honor of Barnett's colleague Dr. C. G. Drake as a Professorship in Neurosurgery.

London's University Hospital opened in 1971. Its entrepreneurial attitude, its provision of 58,000 square feet of research space for a hospital initially with fewer than 400 beds, and its demand for a geographically full-time professional staff who would limit their incomes and contribute any excess above a fixed ceiling to Departmental research budgets, proved to be an ideal climate in which to start the engines which would drive this new Department. The two traditional and more community-oriented London hospitals became key units in the new department, adding a base of clinical expertise and patient resources essential to much of the clinical investigation which began to flow from the Department. The combination of Drake's calm, quiet wisdom, coupled with Barnett's restless energy and impatience to cut through bureaucratic but traditional organizational methods, was the right combination to fashion a springboard for the launching of this collaborative and focussed research.

In 1969, a few short months after Drake had lured Barnett to London, and as the new Department was undergoing active recruitment, it was visited by three aggressive McMaster Medical School scientists: Drs. Fraser Mustard, Jack Hirsh, and Michael Gent. They proposed a conjoined residency training program. This did not materialize. More importantly, they urged a linked research project to evaluate by modern methodological and statistical strategies the possibility of reducing the occurrence of stroke by a double-blind randomized trial of two drugs known from Mustard's pioneering work to inhibit platelet function. The requirement was to test their use in a large number of stroke-threatened patients. Thus was born the first large multi-centre trial of platelet inhibitors and the first large randomized trial for patients threatened with a serious neurological disease. In 1977, the collaborators were able to announce that aspirin prevents strokes and death in this population of patients. Their data were derived from 12 cities stretching from St. John's, Newfoundland, to Victoria, British Columbia. As a sequel to this clinical trail, supported by Medical Reseach Council funding, three other large and several small trials conducted in Europe and USA have been conducted in this field of stroke prevention. All of the large trials have validated the London-McMaster study which has become known as the Canadian Cooperative Study or the Canadian Aspirin Study.

Barnett was designated as the Principal Investigator of this study, but it was a multi-centre trial dependent on the full cooperation of individual neurologists across Canada and of the McMaster Department of Biostatistics and Epidemiology with particular input and guidance from Prof. M. Gent, Prof. W. Taylor, Dr. J. Hirsh, and Dr. D. Sackett. Barnett regards this type of clinical study as the natural result of his concern for excellence in academic clinical neurology. If the treatment of the illnesses of mankind are to be determined with surety, this type of work, based on bedside and outpatient medicine, is as important for establishing the scientific validity of new concepts as is the experimental laboratory from which the concepts arise. It is dependent on the marriage of excellence applied to clinical practice with the uncompromising scrupulousness of the combined sciences of epidemiology, biostatistics, and methodology. None of these disciplines will solve problems without expert clinical input and vice versa. In acute illness, with a very predictable outcome, an innovative treatment can soon be evaluted in a few patients treated by careful and observant individual physicians. By comparison, in a chronic illness, with a protean course such as that known to exist for patients with stroke-threatening symptoms, outcome events may be months and years away for a majority of patients. To evaluate an innovative or an unproven treatment for these illnesses, patients must be invited to join prospective trials. They must all receive any treatment proven to be valuable beyond reasonable doubt. A segment of the group must receive as well the treatment which is hypothetically of additional value. To ensure validity, freedom from bias, and statistical significance to the analysis of the results, the assignment to the treatment groups must be done by random selection. There must be an adequate number of individuals in each treatment group to answer the question of benefit and a larger number if specific sub-group analyses are desirable. In many instances such a large sampling of patients must come from many clinical units so that multi-centre cooperation has to be achieved and maintained for the several years of follow-up which are needed.

Several disparate types of experts are needed in such studies: clinicians, pharmacologists, statisticians, to mention a few. The clinical expert, to paraphrase Horace, must "play the part of a whetstone, which can make steel sharp, though it has no power itself of cutting". The steel could be regarded as the incisive skill of the statistician called upon to deal with the accurate data provided by the clinical investigator or it could be symbolic of the newly developed therapeutic agent.

The cooperative program, commonly referred to as the Canadian Aspirin Study, probably did more than any other single research effort to arouse interest in Canada in stroke research. The birth of the Canadian Stroke Society was a direct outgrowth of this team effort. At Barnett's urging from a position on the Directorate of the Ontario and of the Canadian Heart Foundations, these bodies embraced stroke research and put scientists committed to stroke research on their Boards and Research Committees. Again at his urging, and that of colleagues, particularly Dr. Vladimir Hachinski, they have been gradually changing their names (e. g. , The Ontario Heart Foundation became The Heart and Stroke Foundation of Ontario and the Canadian Heart Foundation has become The Heart and Stroke Foundation of Canada) to signal to the profession and to the public alike, their commitment to the prevention and treatment of cerebral vascular diseases in addition to those of the heart.

The intensive study required of every patient with transient stroke symptoms, a demand of the protocol of the Canadian Aspirin Study, led to some new knowledge about causation of stroke. A recently identified, but usually benign cardiac entity, the prolapsing mitral valve, was found by Barnett to be a cause of stroke. Further epidemiologic, pathologic and clinical studies have confirmed that this mechanism of stroke is more common than any other single cause in patients under 45 years of age. The close scrutiny of patients in the Canadian Aspirin Study also identified arteriosclerotic disease in the external carotid artery and thrombo-embolism from the occluded "stump" of the internal carotid artery as mechanisms of stroke in patients with occluded arteries. It had been claimed that patients who escaped stroke when an artery became occluded were no longer at risk for further stroke occurrence in the territory of this occluded artery. These publications by Barnett and his colleagues documented the proof that this was erroneous and that each year 5% of such patients experience a stroke in this territory mostly by the mechanisms which they described from the external carotid artery and the "stump".

The Canadian Aspirin Study was conducted between 1970 and 1977. During these years a dramatic decline was registered in stroke occurrence and mortality. Europe, as well as North America, recorded this remarkable phenomenon. It was not attributable to aspirin usage but probably related to altered dietary habits, the control of blood pressure, and substantially improved abilities to manage and prevent heart disease. Such a decline has imposed a need to look carefully at the true benefits of any new or old strategies to prevent stroke, especially those which, in themselves, carry a risk of triggering stroke. All are agreed that up to now we have no unequivocal evidence about truly effective measures designed to improve brain function after a stroke has declared itself. While these prospects are being actively pursued, clinicians, including Barnett, who are concerned with the great burden imposed on mankind by cerebral vascular disease have concentrated considerable effort on preventive measures.

The introduction of the operating microscope led to the development of exciting microvascular surgical methods. In the 1960's it was shown that it was feasible for those trained in this technical skill to anastomose scalp arteries to surface brain arteries and thereby to deliver measurable quantities of blood to the brain. This led to early optimistic claims about the use of this new variety of "bypass surgery" for the benefit of stroke-threatened patients. It was hoped, and indeed claimed, that this procedure would prevent stroke in those patients in whom the arteries in the neck were already known to be occluded, or in whom the arteries had been shown to be stenosed within the skull and thus beyond the reach of more conventional reparative surgery. Physicians interested in stroke prevention, surgeons able to do this delicate surgery (using a suture smaller in diameter than a human hair), government agencies concerned about rising health care costs, as well as patients with this combination of arteriosclerotic disease and threatening symptoms, all needed a firm answer about the possible benefit of this new type of surgery. In 1977, Barnett was invited by the National Institute of Neurological, Communicative Disease and Stroke (NINCDS) to put together a team with the expertise to obtain the answer to this burning question and to apply for a competitive grant from NINCDS, a division of the National Institutes of Health of the Department of Health and Human Services of the United States.

Barnett turned to his neurosurgical colleague, Dr. S. J. Peerless, and between them they assembled a North American team of experts in neurology and of surgeons skilled in this new surgical method who had the determination to perform a disciplined study. This would require the full knowledge and cooperation of their patients. The trial required the investigators to randomize half of the patients to the surgical group plus best medical care and half to the best medical care group alone. The McMaster team were asked to cooperate again: Drs. David Sackett, Brian Haynes, and Wayne Taylor and their staff made invaluable contributions. To fulfill the initial sample-size calculation of a minimum of 1,000 patients, the study enlisted 39 North American medical centres, but within a year, had recruited 19 centres in 9 European countries as well as 13 in Japan and Taiwan. For 9 years the total of 1,377 patients who were ultimately accumulated were followed at 3-monthly intervals. Despite their location on three continents, none were lost and all received an average of 5 years of follow-up. In 1985, it was possible to announce that the procedure did not prevent stroke. Debate soon raged, fomented by those disappointed with the resoundingly but unexpectedly negative results. However, 4 years after the negative announcement was made to the World Congress of Neurosurgery, the procedure was performed at a level 95% below what it was estimated would have been the case had there been no trial. Several times the trial has been held up to the Congress of the United States as a model of the cost-benefit of medical research. For an annual investment of 1 million dollars over a 9 year period, a saving of approximately $250,000,000 per year has been realized to the US health care system alone. More importantly, alternative strategies directed toward the prevention of stroke have had to be sought and new research avenues explored. Further investigation using this particular surgical technique has appeared to be futile. Certain spin-off results arose from the very large data-base accumulated in this lengthy prospective study. Some new light was shed on the problem presented by the continuing threat of stroke in the arterial territory of an artery already occluded. The pathophysiological basis for many of these further events appears now to be thrombo-embolism rather than a hemodynamic mechanism. New blood supply failed to prevent the development of events previously claimed by many observers to be due most commonly to the alternative possibility of insufficient flow. Once again "traditional wisdom" has come under serious renewed questioning.

In 1986, another request came for another major trial to be headed by Barnett. Several circumstances led him to launch this major investigation of the surgical procedure of carotid endarterectomy. He was becoming increasingly concerned about the increased use of this surgery, which he had introduced into Canada at the

Toronto General Hospital in the mid 1950's with his surgical colleague Dr. William Lougheed. It had never been properly evaluated by acceptable scientific standards, and was being done too often in an era of declining stroke occurrence and mortality. As Editor-in-Chief of STROKE (the official Journal of the American Heart Association, dealing with cerebral vascular disease), along with influential colleagues in the realm of stroke research and treatment, he supervised the publication of manuscripts which focussed on this concern and was in a position to make critical editorial comment. These publications highlighted the fact that this vexed problem must be placed under the bright light of critical scientific scrutiny. The health-care providers in the United States were worried that this procedure of unproven value was being done at a rate in excess of 100,000 times per year in that country alone. Data became available from the National Hospital Discharge Registry that the operation carried a 10% likelihood of producing stroke or death in those submitted to it in the United States. In 30 years it had been performed everywhere in the world approximately one million times on the basis of little more than speculative evidence. The senior officers of the National Institutes of Health again invited Barnett to submit an application for a grant to settle the issue of the benefit of this operation. This study, now well under way, will require 6 years to complete, will enroll 3,000 patients willing to be followed closely for an average of five years (all of whom will receive the best of known standard care and half of whom by random decision will, in addition, receive the operation), involves 48 hospitals in 41 North American academic medical centres, employs 75 full-time assistants, and will cost a total in excess of 20 million dollars. No medical research project of this magnitude has been known to have been launched in Canada and no other foreign research grant of this size has been awarded by the American National Institutes of Health. Once again it focusses on the need for major collaborative effort between departments, institutions, and countries. The direction of the study is from London, Ontario, at the Robarts Research Institute, with quality control divided between Barnett's long-time colleagues at McMaster and a group at the Mayo Clinic.

Barnett has been one of the driving forces in the creation of a new medical research facility in London: the John P. Robarts Research Institute. Medical research in Canada, by tradition, had centered in the Basic Science Departments of the 16 medical schools and been pursued in the wards and clinics of the affiliated hospitals. University Hospital in London, with its peculiarly large commitment to research, and its consequent attraction to research-oriented clinician-scientists, enjoyed a very rapid period of proliferating research activity. It reached the point, a mere decade after it had been opened, from which it was unable to expand or extend further its creative arm. The Medical School, dependent on the priorities and restricted growth potential of a fixed budget, was threatening to become stagnant. Barnett and Drake shared dreams with successive imaginative and far-sighted Board Chairmen of University Hospital: the late Walter G. Blackburn (publisher and broadcaster) and J. Allyn Taylor, retired as the leading executive figure in Canada Trust. Patrick Blewett, President and Chief Executive Officer of UH, Dr. George Connell, President of UWO, and Dean Martin Hollenberg, supported their plans and their restless desire to make a giant leap forward rather than settling into a senior position in the second echelon of Canadian medical endeavor. The end-result was the birth of a unique Canadian institution: linked to but not funded by UWO, and University Hospital; with government money to build and equip it; with its own Endowment Fund to operate it; with a Board of Directors drawn from London, the rest of Canada, USA and Britain, representing able members of the business and scientific community; and with a core of scientists capable of achieving support from peer-reviewing agencies and foundations. Barnett and Drake's interest in stroke were represented by a Stroke and Aging Research Group and a Clinical Trials Resources Group. But the Institute was planned to reach beyond these areas with four other Research Groups: Heart and Circulation, Immunobiology, Advanced Imaging, and Clinical Pharmacology. Unlike all other major medical research facilities in Canada, it supports its 58,000 feet of modern research facility without ongoing government money and all indications are that it will be a prototype for other privately-run institutions associated with, but not directed nor funded by medical schools, universities, and hospitals. In 1988, Barnett was appointed to his second term as Scientific Director and Chief Executive Officer of this Institute. By telling and retelling the public of the Institute's goals and aspirations, a climate of enthusiasm for its development has become evident. Its autonomy has troubled University and Medical School administators, but this understandable reaction will subside as the Institute's achievements justify this independent status.

Several things in Barnett's career are neither orthodox nor traditional. His scientific training is simply that of a bedside clinician and yet he has been active in scientific research which has changed the practice of his speciality throughout the world. The use of the randomized trial in the evaluation of neurological therapy, and especially of surgical procedures affecting the blood supply of the brain, has been singularly non-traditional, but shows signs of becoming the accepted pattern. Collaborative activity—interdisciplinary, interdepartmental, and international—has been introduced to the study of stroke and threatened stroke.

Barnett's clinical work has been of sufficient interest to him and his colleagues that he has been moved to write 82 scientific papers and an equal number of book chapters; some of these have described new clinical entities, some have shed new light on the mechanisms of cerebral ischemia, and some have merely synthesized progress in these areas. He has edited and written seven books, one of which is going into its second edition as the standard two-volume reference work on "Stroke: Pathophysiology, Diagnosis and Management". He has been invited to deliver 26 named lectureships. Unlike his fellow Killam Laureates, his contributions have not been through the introduction or utilization of wet laboratory research, or complicated technology. When he had developed his skills as a practitioner of medicine in the field of neurology, he simply structured his work and asked colleagues on three continents to so structure their care of sick patients that they could, collectively, determine answers to important therapeutic problems. Along the way, he and his collaborators have had the opportunity to compile large amounts of data related to the illnesses of the patients under study. Clues about the processes responsible for stroke have emerged in scientific papers from these rich data-bases.

A wife, four children, and eleven grandchildren have enriched his life in which many pre-dawn hours, most weekday evenings and many weekends have been occupied with the patients, "The Department", the Journal, the manuscripts, the current Study, or the new Institute. The rewards have been many. The American College of Physicians and the Royal College of Physicians (England) each have awarded him a Fellowship, as has the Medical Society of London; Dalhousie University, an Honorary Doctorate of Laws; the New York Institute of Technology, an Honorary Doctorate of Science; the Royal Bank Awards Committee its Royal Bank Award (shared with Dr. C. G. Drake); the Governor-General of Canada, an Officer of the Order of Canada; and now the Canada Council, the Izaak Walton Killam Award.

The pace of life at times has been hectic. Lectures on his subjects of special interest have been requested in many places and have been coupled often with the pursuit of data being collected for collaborative studies. He has been Visiting Professor in all Canadian and the majority of major American medical schools, as well as many in Europe, the Middle East, South America, and Asia. The sheet-anchor for this busy life has been a marriage to a remarkable woman, a social life largely related to family, and a country retreat to which he repairs most weekends to indulge in the simple pleasures of gardening, cross-country skiing, skating, tennis, logging for fire-wood, and of course bird-watching. In later years some of these pleasures have been shared with grandchildren, of whom there are now eleven. His two daughters, Anne and Jane, both have degrees in English, have literary talents and help to direct his non-medical reading. His two sons, William and Ian, have followed scientific careers, the former as a gastroenterologist in London, Ontario, interested in peptide research, the latter as a field biologist in British Columbia, the career once aspired to by his father.

Barnett's career has not been usual for either a practitioner or a neuroscientist. As a clinician he is aware of the urgent need to bridge the gap between the basic science of medicine and its practice. Barnett's way is not the only answer to this need, but it has been productive and stimulating. He perceives certain drawbacks to a career in clinical science in which the main ingredients are clinical study and structured therapeutic investigation. In the first place, it is demanding of time and energy. Secondly, because basic research work must be done in order that its benefit for patients can be scientifically evaluated, the clinician-investigator depends on previous work and present cooperation. Thirdly, there remains a considerable, albeit declining, body of detractors, who eschew the "mere practice of medicine" as representing a career which a "real scientist" would not follow. Happily the pleasures and the rewards of discovery, although differing between the laboratory and the clinic, are not lacking in either location. Fourthly, there is a long time-lag between the beginning of a major clinical study and the final answer. There is sufficient time available in the career of any individual clinical person to tackle no more than a very few problems and see the studies through to completion. Finally, disappointment must be anticipated. As in any other scientific pursuit, negative or inconclusive results may emerge from a long and tedious period of enquiry. Obviously it is not likely that any scientist, clinical or otherwise, will tackle a problem in which the probable answer is foreseen as a negative one. However, pilot studies and animal research both can be misleading and neither can be used to apply a promising treatment or strategy at the bedside. If the human experience following an expanded clinical application is unfavorable, or if the innovation carries what proves to be an unacceptably high risk, the unwanted result must be accepted and a new direction pursued. The clinician who devotes many long years to a trial which has results which may be positive but may prove, unfortunately, in the end to be negative, must be patient and console himself with the knowledge, as Rene Levesque once put it in the political arena, "that flowers cannot be made to grow faster by pulling on them".

Barnett believes that clinical investigators enjoy a number of very special and particular privileges. There is the privilege of going to the clinic and the bedside to consult with patients about their illnesses. The prospect of this association is what lured most physicians to study medicine and it yields satisfactions which few other vocations can match. It is very challenging to be invited by trusting sick persons and their families to analyze their symptoms and endeavor to restore them to health. When this challenge can be coupled with the possibility of adding to the knowledge of disease and its management, it bestows an additional sense of accomplishment to the practice of medicine. The clinician is privileged to chart his own professional destiny. Despite the fact that the practice of medicine requires a good deal of self-discipline and attention to professional and academic regulations, the practitioner enjoys the exhilaration of individual enterprise less restrained by the types of barriers imposed by a more routine institutional life. Inhibiting and soul-destroying limitations are common in the worlds of education, big business, and also alas in some places in the academic world. Nevertheless the "Syndrome of La Verendrye" does not need to afflict the clinician in the same way that it interferes in other areas of human endeavor. This great explorer of Canada's western expanses was able to glimpse once the "Shining Mountains" in his futile 45-year pursuit of the "Western Sea" that was believed to lead to the Orient. His goal was never achieved. Regulations and limitations were imposed upon him by others who did not know the problem, and these constraints always brought him prematurely back to Montreal; the time allowed him by his masters for each individual foray had elapsed. The directives from Paris aborted each and every one of his valiant attempts. By contrast a clinician has a considerable freedom more comparable to that enjoyed by Alexander MacKenzie, Simon Fraser, and David Thompson. Untrammeled by any unnecessary rules and regulations that were not of their own making, they had reached La Verendrye's and their goal within a decade and had mapped the MacKenzie, the Peace, the Thompson, the Fraser, and parts of the Columbia rivers all the way to their mouths in the Pacific and Arctic Oceans. Freedom exercised with reason and responsibility outperforms a career beset with constraints, with regulations and restrictions.

Barnett believes that physicians must accept the right of the public to be kept informed about the directions and achievements of medical research. Indifference to this responsibility will deny an invaluable ally to the future of scientific pursuit: a well-informed and concerned public. The non-medical public has become medically sophisticated and has developed an increased interest in health care issues. The clinical investigator is commonly invited to explain exciting medical research advances to the public. He is also obliged to stimulate public interest in private and governmental financial assistance for medical research. He must be prepared to take the research message to the political and business leaders of the country. These approaches must be conducted in a fashion which is never designed to enhance the individual's personal reputation, and must make no claims nor promises which are outside the realm of reasoned possibility.

Barnett feels that certain pitfalls in the realm of personal behavior must be watched lest they beset the practicing physician, diminish productivity, and deny the pleasures and true rewards of scientific medicine. Patients, as well as colleagues, frequently place the expert in clinical medicine on a pedestal. Barnett is aware that this may lead the physician to undesirable degrees of pride. He has frequently advised his young associates to remember to take their work seriously, but not to take themselves too seriously. Vanity, jealousy, greed, pettiness, and at times bitterness, are destructive temptations for the clinician, just as they are for those in other scientific pursuits. Unenlightened self-interest, described as the theme in Barbara Tuchman's "March of Folly", may not be easier for the clinical researcher to avoid than it was in the days of the Trojan Wars, or in the days of repressive colonialism. However, a successful professional career is sufficiently rewarding to be worth the consious effort to remain aloof from these common expressions of human frailty.

WILLIAM-HENRY GAUVIN

(March 30, 1913 –   )

Killam Laureate 1988

Le mariage de la science et de l'industrie[1]

Avec un soupçon de génie, le savant peut transformer la matière. William-Henry Gauvin a choisi d'étudier l'état de la matière et de l'énergie des étoiles: le plasma. Cet ingénieur, chercheur et inventeur n'a pourtant pas la tête dans les nuages, mais les pieds bien sur terre; ses plasmas, bien sûr, n'atteignent pas des températures stellaires de millions de degrés, mais seulement de 30 000 à 40 000 °F, ce qui est suffisant pour réduire toutes les matières terrestres à l'état de gaz. William-Henri Gauvin a mis ces plasmas au service de la technologie industrielle.

Rares sont les ingénieurs qui ont réussi l'exploit de mener une carrière universitaire tout en travaillant dans le milieu de l'industrie. William Gauvin est un de ces hommes remarquables. Professeur à l'Université McGill et directeur de nombreux chercheurs qui ont été formés par lui, il a fondé et dirigé pendant plus de 20 ans le Centre de recherche Noranda, un important établissement de recherche et de développement industriel au Canada. Ses travaux en génie chimique ont porté sur la mécanique des fluides, en particulier sur l'étude des principes fondamentaux du transfert de masse et de chaleur, et sur la technologie des plasmas thermiques où il est considéré comme un pionnier.

La recherche fondamentale ne l'a intéressé que dans la mesure où elle permettait de mieux comprendre les phénomènes qu'il essayait de maîtriser. « Pour moi, dit-il, la recherche doit avoir un but bien défini. Le progrès doit continuer. » Cette philosophie propre aux hommes d'affaires a toujours été celle de « Bill » Gauvin, comme il se fait appeler. Depuis 60 ans, il observe avec attention l'évolution du Québec: « Si l'essor primordial est attribuable aux anglophones, les Canadiens français ont su remonter le courant et se battre avec les mêmes outils. Je n'ai pas moi-même souffert d'un sentiment d'infériorité comme un francophone. Je suis trop batailleur et sarcastique. On ne pouvait m'attaquer. Étant à la fois universitaire et industriel, quand je parlais, je mettais l'un ou l'autre chapeau et, la plupart du temps, on m'écoutait. »

L'inspiration de Goethe

Né à Paris en 1913, d'une mère belge et d'un père canadien, William passe une enfance turbulente à Londres, puis à Paris après la guerre. À l'âge de 13 ans, ses parents partent au Canada, laissant pour quelque temps leur fils chez ses grands-parents à Bruxelles. Il entre alors à l'Athénée Royale d'Ixelles, une école connue pour sa discipline et où se donnent des cours de sciences et de mathématiques assez avancés pour l'époque. Il se souvient d'un bouquin portant le titre, inusité pour une école francophone, de *Selfhelp*, utilisé comme texte pour un cours de morale qui l'a profondément marqué. On y présentait un résumé des vies des grands personnages, surtout des philosophes et des scientifiques.

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[1] Cette biographie est adaptée du texte tiré du livre *Une Passion: la SCIENCE: Portraits de pionniers québécois* par Claire Chabot (Éditions MultiMondes, 1990), avec permission.

« C'était toujours le même thème, raconte le chercheur. Tous ces grands hommes avaient commencé de façon très humble et avaient surmonté des obstacles accablants, pour finalement réussir à percer. La vie de Goethe m'a fortement impressionné. Je me suis dit que même si je n'étais pas un génie, je pouvais être un bon élève et que je serais capable de faire quelque chose de bien. » Le jeune homme ne pense qu'à faire une carrière scientifique, tout en s'adonnant à ses passions: les échecs et le piano. Quelques années plus tard, de retour à Paris, il entre au Conservatoire de musique et pratique le piano huit heures par jour, tandis que le soir, il se prépare aux examens d'entrée des grandes écoles. C'était en 1929. Le krach économique anéantit ses projets: ses parents sont ruinés. À 17 ans, William Gauvin part les rejoindre à Montréal.

La décision de devenir ingénieur chimiste est déjà prise depuis longtemps. Mais avant tout, il fallait rétablir les finances de la famille, ce qui lui permettrait d'aller étudier le génie. William est déjà un bon homme d'affaires. « Je me souviens, raconte-t-il, d'être allé à la banque pour demander un prêt de 100 000 $. Mon père, un peu poète, était directeur de nombreuses compagnies (toutes en faillite) et il avait accumulé des dettes s'élevant à 50 000 $, un bon montant à l'époque. Après trois mois de refus, le gérant m'a accordé le prêt. »

Huit ans plus tard, la situation financière se rétablit, et sans dévier de son but, il peut enfin entrer à l'Université McGill. « Les cours qu'on y donnait en calcul intégral et différentiel et en physique étaient les mêmes que j'avais eus à 14 ans en Belgique. » Il est donc entré en 3^e année et sortit deux ans plus tard avec son diplôme d'ingénieur chimiste.

Un début de carrière explosif

C'était en 1941: la guerre battait son plein en Europe. Le jeune ingénieur veut s'engager, mais l'université lui demande plutôt de se joindre à une équipe de recherche sur les explosifs, une commande du ministère de la Défense nationale. Avec ses collègues, il entreprend des recherches sur le RDX, un nouvel explosif d'origine italienne qui avait causé beaucoup de ravages dans la guerre sous-marine, et obtient sa maîtrise. « J'ai essayé, raconte William Gauvin, de m'engager dans l'aviation après ma maîtrise. J'ai affirmé que je n'avais aucun diplôme, mais le major a vérifié. Il m'a engueulé. « Votre boulot, c'est de continuer votre recherche, m'a-t-il dit. » »

Bill Gauvin est alors retourné à l'université pour préparer son doctorat. Dans les années 40, il était le seul à vouloir obtenir un doctorat en génie chimique. On le lui refuse; son Ph. D., il va le recevoir du Département de chimie. C'est pourquoi, quelques années plus tard, devenu professeur à l'Université McGill, il se battra pour admettre des étudiants au doctorat en génie chimique. Après avoir dirigé onze thèses de maîtrise, il faut enfin crier victoire! Son premier étudiant reçoit un Ph. D. en 1954.

Durant ses études au Département de chimie, Gauvin s'était lié d'amitié avec Howden Horner, le fils du président des produits pharmaceutiques Frank W. Horner. Peu de temps après, celui-ci meurt et le fils, Howden, hérite de l'entreprise à 25 ans. « Je l'avais impressionné, raconte Bill Gauvin, et il m'a engagé comme intendant de l'usine. Ce fut une expérience formidable qui a orienté ma carrière. » Howden ne voulait pas, comme son père, se contenter de fabriquer des sirops qui n'étaient rien d'autre que de l'eau colorée à laquelle on ajoutait des vitamines. Il désirait synthétiser de véritables produits pharmaceutiques, basés sur une recherche approfondie, et c'était à William Gauvin de mettre au point la production.

En 1946, un représentant du gouvernement fédéral se présente à l'usine pour s'enquérir de la possibilité de faire sécher du plasma sanguin. Le Canada envoyait alors des milliers de gallons de plasma en Europe pour les soldats canadiens. « Ceci m'a donné une occasion inespérée de mettre au point une nouvelle méthode de séchage à grande échelle, à partir de principes déjà établis, mais très rudimentaires », raconte le chercheur. Avec cet appareil, on pulvérisait le plasma sanguin, la partie liquide du sang, qui était ensuite reconstitué avec une solution saline. « C'est alors que je suis tombé sérieusement malade. Le directeur du Département de génie chimique à McGill est venu me voir à l'hôpital et m'a convaincu de revenir à l'Université. J'ai continué mes recherches, tout en vendant ses *spray dryers*. Je gagnais très bien ma vie comme inventeur et conseiller en séchage. Après un an, j'ai dit au directeur du Département: « Garde les 5 000 $ que tu me donnes en salaire pour développer le laboratoire. » »

Des médicaments aux pâtes et papiers

« J'ai eu plusieurs coups de chance dans ma vie, confie William Gauvin. Je m'en rends compte avec le recul de mes 77 ans. Un jour, par exemple, je suis allé donner un coup de main à un de mes amis à l'Institut des pâtes et papiers quand un grand type entre dans le laboratoire et se présente. C'était le nouveau président de l'Institut. On a bavardé longtemps, puis on a pris un verre et on a dîné ensemble, et à minuit, j'étais en charge d'un nouveau projet de recherche conduit à partir de l'Université McGill. Cela m'a lancé dans un programme de recherche entièrement nouveau, que j'ai poursuivi durant le reste de ma carrière. » L'ingénieur-chercheur, en effet, a mis au point une méthode appelée « technique de suspension atomatisée » (TSA) et qui ressemble à son système de pulvérisation. Il en a étudié les mises en application possibles dans le secteur des pâtes et papiers et obtenu 16 brevets, qui sont aujourd'hui périmés, même si la technique est encore utilisée.

Vingt-cinq ans plus tard, son expérience dans les plasmas permet à Gauvin de retourner à l'Institut des pâtes et papiers pour diriger une autre application, cette fois dans le domaine de la récupération des valeurs chimiques et énergétiques contenues dans les liqueurs noires, résidu qui provient du procédé Kraft pour la production de la pâte à papier. La récupération de ces valeurs est vitale pour le succès économique du procédé, lequel est d'une importance considérable pour l'industrie canadienne.

Il est intéressant de suivre le développement de la technique TSA au cours des années 1950–1965. « Au début, raconte William Gauvin, je prenais la solution contenant des produits chimiques à récupérer et je la pulvérisais, ce qui provoquait une suspension de fines gouttelettes qui séchaient très rapidement par contact avec des gaz extrêmement chauds dans un réacteur cylindrique tout simple. La technique était bonne, mais requérait des températures élevées. Au début, j'utilisais la résistance électrique d'éléments chauffants, mais ce n'était pas suffisant. C'est alors que j'ai entendu parler des arcs électriques et je me suis dit: « C'est ça qu'il nous faut. » L'utilisation des arcs électriques pour produire des plasmas thermiques a commencé en Norvège, avec la fabrication d'ammoniac, et à plus grande échelle, dans les années 40 en Allemagne, où on s'en servait pour produire de l'acétylène.

Les plasmas thermiques sont considérés comme le quatrième état de la matière. Ils sont produits sous forme de gaz à haute température partiellement ionisés. L'élévation de la température du gaz est produite par le transfert d'énergie provenant de l'arc électrique. La technique de suspension atomatisée donnait son plein rendement avec l'utilisation des arcs électriques, lesquels peuvent dégager une énergie énorme. William Gauvin devint alors le spécialiste canadien des plasmas industriels. Il travaillait à des températures de 15 000 K!

En 1961, la compagnie minière Noranda veut ouvrir un centre de recherche. Le président de l'époque, John Bradfield, veut avoir un directeur de recherche; il vient trouver William Gauvin. Lors de leur première rencontre, le chercheur a l'impression que la création d'un tel centre est surtout une question de prestige pour Noranda, et les contraintes que peut faire peser la structure imposante d'une entreprise de la taille de Noranda l'effraient un peu. Suivent quelques mois de silence, puis les principaux dirigeants de la compagnie, presque tous des ingénieurs de haut calibre, reviennent à la charge, assurant William Gauvin qu'il aura une liberté complète. Celui-ci remet un rapport sur différents sites possibles dans six villes canadiennes (tout en souhaitant fermement rester au Québec).

« Je savais qu'ils allaient choisir Montréal, raconte l'ingénieur, mais où? Je les ai baladés en autobus par une chaleur écrasante dans des endroits épouvantables et, vers 17 h, je les ai amenés à Pointe-Claire. Fatigués, ils étaient tous assis dans l'herbe. Ils ont dit: « C'est ici! » Alors j'ai demandé à un collègue d'aller chercher les bières bien glacées, cachées dans les broussailles. Et là, ils se sont rendus compte que j'avais triché. »

« Les mois qui suivirent, poursuit Gauvin, ont été l'occasion d'une expérience inoubliable. Ce fut la collaboration avec les architectes dans le dessin des laboratoires et de l'usine pilote (le cœur de l'établissement), et la sélection de mes futurs chefs de département, dont Noranda m'avait donné la pleine responsabilité (tous des types dynamiques, énergiques, calés dans leur profession, et dont un bon nombre sont encore employés au centre actuel). Un gros problème était de mettre tout ce monde au travail durant la construction des laboratoires. On les a mis un peu partout, à l'Université McGill, dans les laboratoires de la Canadian Copper Refinery, dans des bureaux loués rue Sherbrooke. On en a aussi profité pour leur faire visiter un tas de mines et d'usines de transformation. Finalement, quand le Centre a ouvert ses portes, en 1963, nous étions déjà 35 personnes. »

Le chercheur industriel

Sous la direction de Bill Gauvin, le Centre de technologie Noranda a connu un essor considérable dans le monde minier et métallurgique. L'investissement de la compagnie dans la recherche et le développement de nouveaux procédés engendrait des bénéfices trois à quatre fois plus élevés. « Un nombre considérable de brevets ont été déposés à mon nom et à ce lui de mes chercheurs, relate-t-il. Aucun projet d'importance n'était poursuivi, si sa rentabilité éventuelle n'était pas établie par une analyse fréquente des données de la recherche, au fur et à mesure que progressait celle-ci. Quand les résultats commençaient à devenir défavorables, le projet était abandonné. »

Tout au long de sa carrière comme chercheur industriel, William Gauvin est resté lié à l'Université McGill. Il ne donnait plus de cours, après 1962, mais dirigeait des étudiants à la maîtrise et au doctorat. À l'époque, il y avait au Canada très peu d'ingénieurs œuvrant dans l'industrie, qui s'engageaient dans le milieu universitaire. Gauvin a encouragé ses ingénieurs à donner des cours ou à diriger de la recherche à l'Université McGill. Au début, Noranda ne voyait pas d'un très bon œil ces activités universitaires, mais comme elles étaient conduites sur des sujets d'intérêt pour l'industrie, elle a fini par les approuver. Mais il n'était pas question de prendre du temps de travail pour le consacrer à des étudiants, si prometteurs fussent-ils. « Il était un conseiller avisé pour ses étudiants et pour ses ingénieurs, affirme Peter Tarassoff, l'ingénieur qui lui a succédé . Il était aussi un chef de file très humain, qui encourageait notre initiative. Si le Centre jouit d'une grande réputation dans le monde métallurgique, entre autres en ce qui concerne le développement de procédés de transformation des minerais, c'est grâce à lui. »

Parallèlement à son travail d'ingénieur et de chercheur, William Gauvin a siégé, durant les années 70, au Conseil des sciences du Canada et au Conseil de la politique scientifique du Québec. Cette expérience a fait naître chez lui un intérêt pour l'environnement. Comme aujourd'hui, le domaine minier, à l'époque, ne jouissait pas d'une très bonne réputation auprès des écologistes. Avec son esprit scientifique et surtout sa vision économique, Bill Gauvin écartait l'idée de tout remettre en question, mais voyait la direction à suivre dans le développement de la recherche. Il a compris que tout progrès scientifique et technologique, quel qu'il soit, cause des perturbations et a des impacts sur différents plans. Désormais, il faudrait examiner ces impacts. C'est une philosophie qu'il a mise en pratique chez Noranda. On étudiait particulièrement les effets surs tels que l'intégration des employés, le besoin de nouveaux talents, l'échelle d'avancement. Ce Centre de recherche accordait également une importance particulière à l'environnement: l'épuration de l'air et de l'eau, la gestion des déchets, etc.

Une technologie victime du marché

« Le fameux procédé Noranda mis au point au Centre de recherche, le traitement des concentrés du cuivre, dégageait beaucoup de dioxyde de soufre, explique le chercheur. Après l'étude d'impact, la compagnie a récemment décidé de convertir ces gaz en acide sulfurique, ce qui entraînait un investissement énorme. Qu'est-ce qu'ils vont faire de l'acide? Je ne le sais pas, mais ils le font comme de bons citoyens. »

Les recherches de William Gauvin sur les plasmas industriels, et la collaboration d'un de ses anciens étudiants et chef de département au Centre, George Kubanek, ont mené à l'invention d'un four à plasma de conception nouvelle. Le procédé suscite beaucoup d'intérêt dans le domaine métallurgique. En effet, il peut être utilisé pour la fonte des metaux, l'extraction de minerais, la production d'alliages et le traitement de rebuts métallurgiques. Une des applications que pouvait en tirer la compagnie Noranda était la production de ferromolybdène, dérivé de la molybdénite, minerai dont la compagnie possède plusieurs gisements.

Mais au moment où Noranda considérait la construction d'une usine de production, le marché de l'acier inoxydable, dans lequel cet alliage est employé, s'est effondré, et le projet de l'usine est resté en plan. Malgré ses avantages dans beaucoup d'applications métallurgiques, il faut reconnaître que la technologie des plasmas est assez dispendieuse et ne serait pas rentable pour des métaux communs, comme le zinc, le plomb ou même le cuivre, qui sont les ressources exploitées par Noranda. N'y voyant plus d'intérêt commercial, la compagnie a décidé de vendre le procédé.

Après une carrière remarquable à la direction du Centre de technologie Noranda, William Gauvin, à 71 ans, prend une retraite plutôt active. Il continue de diriger des étudiants au Département de génie chimique de l'Université McGill, démarre sa propre entreprise et devient consultant de recherche pour l'Institut de génie des matériaux, à Boucherville, et pour l'Institut de recherche d'Hydro-Québec (IREQ). Ce dernier institut venait d'acheter de Noranda le procédé breveté par Gauvin et Kubanek, et pour s'assurer d'un bon transfert de cette nouvelle technologie, on a demandé à l'inventeur de servir de conseiller. Hydro-Québec a dénommé la nouvelle technologie Fours Plasmacan.

Les plasmas industriels au service de l'environnement

Vers 1982, Hydro-Québec cherchait des débouchés pour ses surplus d'électricité. Au même moment, Noranda voulait se départir de la technologie des plasmas industriels. Hydro-Québec a donc donné le mandat à son centre de recherche de trouver de nouvelles applications pour l'électricité. Une division de l'Institut, Les Plasmas Industriels, et, plus récemment, le Laboratoire des technologies électrochimiques et des électrotechnologies (LTEE), à Shawinigan, ont été mis sur pied afin de fournir une expertise scientifique à l'industrie. La première entreprise à avoir bénéficié de la technologie des plasmas est Alcan, pour le traitement des déchets d'aluminium. Avec l'obligation de plus en plus sévère de traiter les déchets industriels, le procédé devrait servir à d'autres alumineries à travers le monde.

Les laboratoires d'essai en plasma industriel de l'IREQ et du LTEE sont devenus des plus importants dans le monde. Plusieurs procédés sont à l'étude, que ce soit pour percer des briques réfractaires, brûler les plaques de granite, fabriquer des torches à plasma de haute puissance ou traiter des poussières toxiques. « En 1982, relate Michel Drouet, directeur des plasmas industriels à l'IREQ, avec le prix du pétrole qui montait en flèche—on prévoyait alors jusqu'à 50 $ le baril—l'utilisation de l'électricité devenait intéressante. Aujourd'hui, on considère que le marché potentiel des plasmas a diminué, si ce n'est des bénéfices dans certaines applications où l'avantage est évident sur le plan de l'équipement, de l'automatisation et de l'environnement. »

C'est d'ailleurs sur le plan environnemental, dans la perspective de l'effet de serre créé par le CO_2, que les plasmas industriels suscitent le plus d'espoir. Contrairement aux hauts fourneaux traditionnels, les fours à plasma ont la capacité, non négligeable au plan énergétique, de démarrer et de s'arrêter instantanément. Remplacer des fourneaux qui fonctionnent au pétrole, au gaz naturel ou, pire au charbon, par des fours à plasma utilisant l'électricité est un défi qu'Hydro-Québec est sur le point de relever.

« Bill Gauvin a beaucoup d'expérience, affirme Michel Drouet. C'est pourquoi on aime le consulter. De plus, c'est un homme très accessible. L'IREQ a bénéficié de son prestige. Avec ses contacts à travers le monde, il nous a présentés à des spécialistes. Mais ce qui nous a le plus servi, ce sont les liens qu'il a su établir entre le gouvernement, l'industrie et les universités. » Le centre de recherche en plasma fait partie des actions structurantes visant à créer des regroupements d'expertise entre l'IREQ et les universités McGill et de Sherbrooke.

À la remise du prestigieux prix Killam pour le génie, décerné par le Conseil des arts du Canada, en 1988, on lui attribuait le concept d'« actions concertées » réunissant les milieux universitaires, gouvernementaux et industriels, dont il a été un promoteur exemplaire.

William Gauvin a d'ailleurs joué un rôle prépondérant dans le développement de politiques de recherche, en acceptant la présidence d'un nombre important d'associations scientifiques et professionnelles, dont le Deuxième Congrès mondial de génie chimique, et en étant membre actif de plusieurs organismes, dont le Conseil national de recherches du Canada, le Conseil des sciences du Canada et le Conseil de la politique scientifique du Québec. Il est aussi membre-fondateur de plusieurs associations professionnelles telles que l'Association des directeurs de recherche industrielle du Québec. « Mais quand c'était bien démarré, confie-t-il, je passais à autre chose. » Et c'est peut-être là qu'on retrouve le plus son caractère de pionnier.

Fort de son prestige et de son expérience, William Gauvin songe maintenant à quitter toutes ses charges administratives et universitaires pour se consacrer à l'établissement de liens entre les universités et les industries, une association qui s'impose de plus en plus au Québec. William Gauvin veut aussi se garder un peu plus de temps pour ses premières amours: les échecs et le piano.

JOHN CHARLES POLANYI

(January 23, 1929 –   )

Killam Laureate, 1988

John Charles Polanyi was born in Berlin, Germany, of Hungarian parents, Michael Polanyi and Magda Elizabeth (Kemeny) Polanyi. His family moved to Manchester, England, when he was four years old. His father, Michael Polanyi, was successively Professor of Chemistry and of Philosophy at Manchester University. John entered Manchester University, from Manchester Grammar School, in 1946, just in time to attend his father's last lectures on the subject of chemistry, given to the first year.

In English universities at that date, "The Prof.'s" interests coloured the activities of the entire department. When, in 1947, Michael Polanyi moved over to the Humanities, he left behind an academic staff that was devoted in large part to the exploration of the molecular basis for simple chemical reactions. John Polanyi's research supervisor was a spirited ex-student of Michael Polanyi, Ernest Warhurst. Warhurst had obtained his doctorate using the "sodium flame" apparatus which had enabled Polanyi Sr. and his co-workers to determine the probability that a collision between a sodium atom and a molecule would lead to reaction. He put Polanyi Jr. to work measuring the strengths of chemical bonds by pyrolysis (thermal dissociation); this study led to Polanyi's Ph.D. in 1952.

John Polanyi credits these years at Manchester University with having supplied him with the right questions to ask. The molecular basis for chemical reaction became the subject of the work for which, thirty-five years later, he received the Nobel prize.

He re-cast the questions that he had heard discussed in his student days so that they provided a fitting challenge for the coming period. In Manchester, the main focus was upon the mystery of over-all reaction rate. Would a molecular collision of a given energy result in the formation of a new chemical species, or would it not? The time seemed opportune to ask, and perhaps answer, the further question: what *types* of forces operating during a molecular collision are the most conducive to reaction? The easiest way to learn something about this would be to study the motions of newly-born reaction products, since the forces operating in the "transition state" (part way between reagents and products) would imprint themselves on the products.

During his postdoctoral work with E.W.R. Steacie at the National Research Council of Canada's laboratories in 1952-1954, Polanyi became increasingly convinced that this was the right question to be asking. He also became convinced, for the first time, that he would like to be the person asking the question. His sense of vocation had been slow in coming. As a student at Manchester, he was interested in politics (editing a newspaper) and in writing (a narrative poem that he wrote as a graduate student covered a page in the New Statesman and Nation, to his delight) but only peripherally interested in science.

In Ottawa at the National Research Council (NRC), he was intoxicated by the experience of living under blue skies and, feeling permanently on holiday, he began to enjoy his work in the laboratory as never before. With Steacie's blessing, he embarked on some calculations to see whether the reigning theory of reaction rates, the so-called "Transition State Theory", had predictive power. He came to the conclusion that the theory was built on quicksand, since knowledge of the forces in the transition state region was lacking. In 1954, at the close of his two years at NRC, he spent a few months enjoying the hospitality of Gerhard Herzberg's laboratory where he assembled spectroscopic equipment to probe vibrational and rotational excitation in molecular iodine. As he put it subsequently, "some unseen hand must have been guiding me", since this work led him further along the way to measuring the corresponding types of motion in reaction products.

The same hand may have guided Polanyi to Princeton, where he spent two further years, 1954-1956, still as a postdoctoral fellow. Though he went there at the invitation of Sir Hugh Taylor, his main interactions were with two associates of Taylor's: Michael Boudart and David Garvin. Together with James McKinley, these workers were studying vibrational excitation in the products of the reaction of atomic hydrogen with ozone. The reaction gave rise to an orange glow as the highly vibrationally excited reaction product made spontaneous transitions directly to very low states of excitation. Though Polanyi did not participate in this experiment, he was influenced by it. If these three workers could see "high overtone" vibrational transitions, then it should be possible to detect the much more probable "fundamental transitions" due to smaller changes in vibrational state. These would give rise to infrared emission. Polanyi took this idea with him to Toronto, when he moved there as Lecturer in Chemistry at the University, in 1956.

With his first graduate students, he looked for enhanced reaction rate in vibrationally excited hydrogen formed in a discharge, and also for vibrationally excited hydrogen chloride in the product of the exothermic reaction of atomic hydrogen and molecular chlorine. It was this second line of experimentation, first reported in the literature in 1958, that set Polanyi and his group on the path to their main work.

The experiment was simple and (for sound reasons) inexpensive. Atomic hydrogen, formed in an electric discharge by power taken from a neon sign transformer, was mixed with a flow of chlorine gas at low pressure in a vessel equipped with sodium chloride windows that transmitted infrared. An infrared spectrometer, shared as an analytic tool by the entire department, was borrowed and positioned in front of the sodium chloride window. With the reagent gases flowing, the reaction vessel, which remained cold to the touch, gave out an infrared spectrum indicative of the presence in the vessel of hydrogen chloride at a temperature of thousands of degrees. This molecular excitation was chemical in origin and constituted "infrared chemiluminescence".

The 1958 communication by Polanyi and his student J.K. Cashion ended with the statement that "The method promises to provide for the first time information concerning the distribution of vibrational and possibly rotational energy among the products of a three-center reaction".

It took a further decade, and the work of a substantial group of graduate students, to fully realise this goal. It was necessary to combat the effects of vibrational and rotational "relaxation". This was achieved most effectively by using sprays of reagent gases that crossed in the centre of an evacuated chamber. The infrared emission from new-born reaction products was recorded. The products were then removed at the liquid-nitrogen-cooled walls of the vessel before they had time to relax. This "arrested relaxation" approach led to the first quantitative determinations of "detailed rate constants", that is to say, the rates of reaction into specified states of product vibrational, rotational, and (hence) translational excitation.

This last item of information — translational excitation — provided common ground between the infrared chemiluminescence studies in Polanyi's laboratory and the crossed molecular beam studies which constituted the major alternative route to such measurements. The crossed-beam method, in which the prime measurables are product translational and angular distributions, came to maturity in the hands of D.R. Herschbach and Y.T. Lee with whom Polanyi shared the 1986 Nobel prize in Chemistry.

A major technological outgrowth of Polanyi's work first became evident only two years after the publication of the initial report of infrared chemiluminescence. In 1960 Polanyi submitted a paper to Physical Review Letters entitled "An Infrared Maser Dependent on Vibrational Excitation". This extended the prior proposal by C.H. Townes and A.L. Schawlow (in 1958) for what today we call a 'Laser'.

Townes and Schawlow had envisaged a medium made up of electronically excited species. Polanyi proposed that the medium be composed of vibrationally and rotationally excited molecules. The suggestion was tempting for a variety of reasons. In the first place, because of a phenomenon that Polanyi termed "partial population inversion", a lasing medium could be obtained by the simple process of (partially) cooling a hot gas. Secondly, a chemical reaction could be used to generate the lasing medium; today this is known as a "chemical laser". Additionally, Polanyi noted that such lasers should exist naturally in the upper atmosphere.

Physical Review Letters rejected the paper as lacking scientific interest. Shortly thereafter they rejected T. Maiman's report of the first operating laser, on the same grounds. Polanyi read about this second rejection, quite by chance, while holidaying on an island in Georgian Bay. On returning to Toronto in September of 1960, he submitted the identical manuscript to the Journal of Chemical Physics, where it was promptly published.

Since then, vibrational lasers and, in particular, chemical lasers (due, respectively, to C.K.N. Patel and G.C. Pimentel) have developed into the most powerful sources of infrared radiation. Polanyi is fond of asking sponsors of basic research who insist on evident promise of applications, whether they would have been far-sighted enough to support studies of barely detectable luminescence as a means to the development of the most powerful lasers in existence.

Despite its simplicity, the infrared chemiluminescence approach provided the most complete and detailed product energy distributions available for any chemical reactions. As a consequence, they were widely used as a testing ground for theories of the molecular mechanics of simple exchange reactions. Polanyi's group was active from the outset of its experimental program in attempting to link the underlying pattern of forces to the molecular motions by means of computer modelling.

The method used depended upon the happy circumstance that high-speed computers made it possible to solve the equations of motion for the reacting particles. The interplay of theory and experiment led to a number of simple insights. In endothermic (uphill) reactions, the energy in the transition state is most effective if it stretches the bond under attack, that is, if it is vibrational. Reagent motion is channelled through the transition state so as to become a similar type of motion in the products (translation into translation, vibration into vibration). Commonly, a molecule can be approached in two different ways to yield chemically the same product; this implies the existence of two different transition states which give rise to two patterns of motion in the product (a phenomenon termed "microscopic branching").

The infrared chemiluminescence approach was not restricted to the determination of product excitation. It could also be used to measure the effect of varying reagent vibrational and rotational excitation on reaction probability. This was achieved by using a prior reaction to generate infrared chemiluminescence and then recording the "chemiluminescence depletion", that is, the extent of reaction from the various states of vibrational and rotational excitation when pulses of a further reagent were introduced.

In more recent times, Polanyi's research interests have moved beyond the studies of reaction dynamics cited by the Nobel Foundation. In one recent line of work, the objective has been to develop what is termed a "spectroscopy of the reactive transition state". The hope is that one will be able to gain an insight into the molecular dance in chemical reaction by observing the molecular partners while they are on the stage, rather than in the wings before or after the dance is over. (This simile is a favourite one of Polanyi's, acknowledged at a gala performance of the National Ballet of Canada in 1987 at which Polanyi was invited, briefly, to observe the ballet from centre stage.)

In a second expanding line of research, members of Polanyi's group are developing a photochemistry of the adsorbed state. With the aid of ultraviolet laser radiation, they induce reaction between neighbouring molecules held at a solid surface. By means of this surface aligned reaction ("SAP"), they believe that molecules can be positioned at will and subsequently be induced to react in a determined way. To the extent that this succeeds, the dynamicist has become choreographer.

In the late 1950's, Polanyi became convinced of the need for scientists to involve themselves in public affairs, as they relate to the problem of survival in an age of nuclear weapons, and to the management of other equally powerful technologies. He has published some seventy articles on this and related questions, and has given many times that number of talks. In 1960 he became the founding chairman of the Canadian Pugwash Group and continued to hold that position until 1978. In 1978 he chaired an international symposium on 'The Dangers of Nuclear War', that led to a book of that title. He has been an active member of the American Academy's Committee on International Security Studies and also of the Canadian Centre for Arms Control and Disarmament.

In a different facet of his interests, he has been a founding member of the Royal Society of Canada's Committee on Scholarly Freedom and of the Canadian Committee of Scientists and Scholars. These activities have left time for involvement in the ongoing Canadian debate regarding science policy from the time of the Lamontagne Report two decades ago to the time of his recent involvement, at its formation, in the National Advisory Board on Science and Technology (NABST) chaired by the Prime Minister. Polanyi has continually stressed that high quality science is a necessary investment in the future. To the cry for "relevance" in basic science, he has responded that "nothing is more irredeemably irrelevant than bad science".

John Polanyi was married in 1958 to "Sue" Davidson (Anne Ferrar Davidson) of Toronto, a musician and piano teacher. John Polanyi describes himself as a musical ignoramus. His aesthetic pleasures come from art, literature, and poetry. He and his wife have written words and music, respectively, for skits which have been performed professionally. In a more active vein, John Polanyi has outgrown his youthful enthusiasm for white water canoeing and aerobatics in favour of skiing and walking. John and Sue have two children, Margaret, a journalist, and Michael, a physicist turned political scientist.

The award of the Nobel prize in 1986 to Polanyi, D.R. Herschbach, and Y.T. Lee was for contributions to "the development of a new field of research in chemistry — reaction dynamics". In particular, Polanyi was cited for "the method of infrared chemiluminescence, in which the extremely weak infrared emission from a newly-formed molecule is measured and analysed", and for his application of "this method to elucidate the detailed energy disposal during chemical reactions". Polanyi has received many honours. A partial list includes the Marlow Medal of the Faraday Society in 1962, the Steacie prize for the Natural Sciences (with

N. Bartlett) in 1965, the Tory Medal of the Royal Society of Canada in 1977, the Wolf prize (shared with G. Pimentel) in 1982, the Nobel prize in Chemistry in 1986, the Isaac Walton Killam Memorial prize in 1988, and the Royal Medal of the Royal Society of London in 1989. His list of named lectureships is extensive. The tally of honorary degrees comprises twenty-three in Canada, the United States, the United Kingdom, Israel, and Italy. The Government of Canada appointed him an Officer of the Order of Canada in 1974 and a Companion of the Order of Canada in 1979. He was made a Fellow of the Royal Society of Canada in 1966, the Royal Society of London in 1971, the Royal Society of Edinburgh in 1988, a Foreign Member of the American Academy of Arts and Science in 1976, a Foreign Associate of the (U.S.) National Academy of Sciences in 1978, and a Member of the Pontifical Academy, Rome, 1986.

Canada has been rather infrequently represented on the platform in Stockholm: once for medicine, once for peace, and now twice for the physical sciences. Consequently, the status of Nobel Laureate carries special rewards in the form of public acclaim and special responsibilities due to public demands. In a speech of March 4th, 1987, the Prime Minister of Canada quoted John Polanyi:

"We in Canada must have flourishing science in addition to vigorous technology — because a nation is more than a machine for creating wealth. Science is the glory as well as the terror of mankind. Our respect for ourselves as a people requires that we, as a prosperous and civilised nation, contribute to this central strand of twentieth century culture."

Polanyi's fervent hope is that one can combine the necessary activity of speaking for science with the more congenial one of doing it.

James Keith Brimacombe, O.C., F.R.S.C.

(December 7, 1943 –)

Killam Laureate, 1989

Life began for me in Windsor, Nova Scotia, two years to the day after Pearl Harbour. The world was at war and my father was a flying instructor with the Royal Canadian Air Force training pilots at Stanley. At the tender age of six weeks, I was bundled up by my mother and grandmother and, with my older brother, Ron, journeyed by train across the frozen expanse of Canada to central Alberta, home to both my parents – Geoff and Jean. My earliest recollections are of this fertile area of Canada where I spent my boyhood in a small farming community edged against a railway line. Rosalind had four grain elevators that collected the harvest of each year and fed it to the outside. Distributing this rich bounty from the land where dinosaurs once roamed was the stream-driven train puffing into the station and snorting vapour into the autumn still and gathering winter freeze. The train was an event punctuating my boyhood. Sometimes in anticipation of its arrival, I would crouch with my junior cohorts and press my ear to the rail and listen for the pinging messages of this behemoth bearing down on the tiny band. We would place pennies on the steel track to test the might of this giant machine - Davids against Goliath? - fearing all the while that our puny intervention would cause a derailment. Instead our pennies were uniformly flattened, copper yielding to steel, leaving only a fingerprint of the original minted image. This rather rudimentary rolling operation was my entry into the world of metals and how they behaved.

Filtered through decades, this seems now to have been a magic time – it certainly was one of unfocussed curiosity. It was a time of poking into gopher holes, climbing tall trees in search of birds' nests, listening for the sound of the first meadow lark in the dawn quiet, smelling the sweetness of cut grass drying in the sun, testing the frozen surface of a bridge girder with your tongue – the stuff of W.O. Mitchell whose writings have since touched me profoundly. Some days I would sit in my grandfather's general store and listen to the overall-clad farmers musing over the weather, crop prices and machinery. My grandparents (maternal) lived next door to our home and, like most of the village people, had a large vegetable plot. Summer mornings would often find me at the break of dawn with my grandfather in his garden. We would work together, him surgically removing weeds with his hoe while giving the boy instructions on hilling potatoes or tying up tomato plants. Throughout my life, I have loved the early morning and the quiet of working in my study, filled with fresh thoughts and anticipation of a new day. My grandfather was a large quiet man with an abiding kindness and concern for others that permeated all that he did. Of Scottish descent (MacDonald), he was born in Prince Edward Island and, as a young man, came to Alberta by train with threshing crews. There he met my grandmother, who had journeyed from Minnesota, and they eventually settled in Rosalind where they raised a large family. Of German stock (Weller), my grandmother was his equal in generosity, kindness, and love of her family. She had to terminate her schooling at an early age to care for her younger siblings, when her mother died. But she was an intelligent woman nonetheless; later, when we moved from Rosalind, I came to value the stream-of-consciousness letters that she wrote filled with all the happenings of her busy life.

My paternal grandparents lived in Vermilion, a larger Alberta town close to the Saskatchewan border, where my father was born, the second youngest of five boys. My grandmother was of French extraction (Plante) but sadly died of a stroke before any of her grandchildren were born. Grandpa Brimacombe came to Alberta from Ontario but the family name originates several generations back in Devonshire close to the border with Cornwall in the West Country of England. He owned a bookstore and loved the written word; without fail, each Christmas he would send my four brothers and me books of stories and poems. Also a quiet kind man, he enjoyed playing catch with us or allowing us to caddy for him on the golf course.

My parents met at normal school (teachers' college) where my father captained the hockey team and my mother led in scholastics. Teaching and education, then, are in the blood. Following the war, my father returned to Rosalind to work with my grandfather in the general store while mother concentrated on raising a family of (soon to be) five boys (two girls were to follow). They provided a warm caring home which had to withstand the tumults of spinal meningitis (brother Neil), scarlet fever (me), and polio (brother Ron and me). Later when my own children, Kathryn and Jane, entered the world, I would want to provide the same home in a different time and in a different place.

So it was a good beginning. If I dwell on this time, it is because it has been the most important. Margaret Laurence, the great Canadian writer, perhaps said it best:

> "The world you were a child in stayed with you all the days of your life. Its light was the light you always saw by and its dark the dark that colored all your days."

It was a time when I learned about such fundamentals as the struggle of man with nature and his bond with the soil. Later I would wonder why I felt such empathy with the writings of John Steinbeck but now I understand. It was a time when I learned of the beauty of human relationships and began to develop a strong sense of myself. My roots were sunk deep in Western Canada although later my studies and work would take me across the country and far beyond her shores.

When I was ten, we moved to Lethbridge on the flat Alberta prairies. There was nothing there to stop the wind blowing off the western mountains. The sight of a chinook arch opening up blue in an otherwise overcast sky in winter, with the sure knowledge that the temperature would soon rise dramatically and the snow would disappear, was awesome. So too was the spectacle of dark clouds of top soil scoured from the land by the persistent surging air. In Lethbridge I entered the Catholic school system which included three years in an excellent boys' high school, St. Francis, run by dedicated priests and lay teachers. Beyond kindling my interest in Mathematics, Physics, Chemistry, and History, they showed me commitment and purpose. Because the school was relatively small, we were encouraged to join the sports teams and I ended up playing basketball and football although neither particularly well. But most importantly I experienced really what sports are all about – teamwork, perspiration, dedication, and a strong desire to win. This has stuck with me even though in those cold autumn mornings, when I dragged my scraped and bruised body off the frozen football practice field in time for school, the activity did not appear to be especially ennobling. This also was a time when I joined the air cadet movement, not because of an interest in war, but a fascination with flying which has remained to this day. Perhaps again it is in the blood since not only my father but five uncles served in the Royal Canadian Air Force during World War II. I enjoyed the camaraderie of working closely with other boys and began to develop a much stronger sense of discipline. My final year of school was spent at Delbrook High in North Vancouver where we moved in 1960. There I had a splendid set of teachers in all my subjects but especially English, History, Mathematics, and the physical sciences. Our English teacher made the subject live and I shall never forget being required in the classroom to play Romeo opposite to a girl, as Juliet, with whom I was quite madly in love. It was a good preparation for university on which my sights were firmly set.

Thus in September, 1961, I entered the University of British Columbia and quickly learned the meaning of the word queue during registration week – queues for courses, queues for changes, queues for books. At that time the university was only about one third of its present size but it seemed very large to me and somewhat daunting. Being a freshman, I wondered how I would cope with a system so different from high school and if I could excel in the competitive environment. But I was also struck by the intellectual atmosphere of the place and its unique beauty set as it is on Point Gray jutting out into the Strait of Georgia. This was where I really wanted to be, where knowledge and the development of the mind were the essence. In my first year of Arts and Science, I was exposed to passable lecturers and soon learned that, finally, it was up to me to fill in the gaps of my understanding. The university motto, Tuum Est – It's up to you, really did apply. Most importantly, I was stimulated to think sometimes in the most unconventional way. My English 100 professor, for example, saw phallic symbols and sexual connotations in every poem and piece of prose we discussed with the one exception of D.H. Lawrence's The Snake! Through all the hard work and long hours, I really began to develop a love for learning which in high school, for me, seemed more of a requirement.

It was in my first year at UBC that I finally decided I wanted to be an engineer more than a scientist, perhaps because I enjoyed the people side of engineering and I liked problem solving. Moreover engineering, with extremely heavy course loads in non-trivial subjects, was an enormous challenge intellectually and physically. I suppose I wanted to test my mettle, and in 1962, I entered the four-year engineering program at UBC. It was as challenging as I had anticipated, perhaps more so, because at times I felt as if I was drowning in lectures, assignments, and study. In first year Arts and Science, I had five courses, while in first year Engineering I had eight (or was it nine?) courses. But survive I did and in second year took on Chemical Engineering with its emphasis on processing which has become the central theme of my research today. With a strong affinity for Mathematics, Physics, and Chemistry, I came to realize that I also wanted to know how this knowledge could be harnessed in the real-world activity of making things via processes.

Although I enjoyed my foray into Chemical Engineering, there seemed to be something missing, a doubt lurking in the recesses of my mind about whether I was cut out to work on chemical processes like oil refining and reagent production. And thus, in 1963, I took on the study of metallurgy which, in one form or another, has shaped the evolution of civilization for at least six thousand years. Perhaps it was these historical roots, or it may have been the searing temperatures and sheer difficulty of pyrometallurgical production, which influenced my decision - and the course of my career – but the next three years at UBC were spent in the Metallurgical Engineering program. There were many surprises and difficulties along the way while I grew intellectually. I was fascinated by the crystallographic order of metals on the atomic scale, and how disorder in crystals profoundly influences their mechanical behaviour. The study of metals at this level with X-rays stimulated my curiousity about their macroscopic behaviour during processing operations like rolling and heat treatment, but it became apparent that this was only crudely, and qualitatively, understood. Later this need to develop microstructural engineering to characterize solid metal behaviour quantitatively became the impetus for part of my research. Another subject which captured my imagination was thermodynamics with its elegance, complexity, and usefulness to describe metallurgical processes quantitatively. I studied the subject in courses having very different approaches – one taught by chemists based on concepts of statistical thermodynamics and the other by a metallurgist, C.S. Samis, whose primary concern was equilibria amongst phases in metallurgical reactors producing iron, lead, zinc, copper, nickel, or any other metal. But the approaches were complementary, the first providing a theoretical underpinning to the problems of metals production, the second giving examples to illustrate theory. This was a wonderful example of pure science and engineering combining to achieve a unique learning experience.

It was not all academic work at UBC because in my last year I jumped in to become President of the UBC Graduating Class as well as President of the Metallurgical Engineering Club – presumably to satisfy my interest in people and the political process. Also, throughout my years as a UBC undergraduate, I was enrolled in the Regular Officer Training Plan of the Canadian Armed Forces which I enjoyed for its emphasis on people, communication, and discipline. Although I resigned my commission of Captain in 1970, this was a most rewarding experience which helped round out my growth.

In my final year at UBC, I was encouraged by Professor Samis to take up the challenge of graduate work and to do so far afield in England. Samis himself had studied at the University of London for his Ph.D. and knew the broadening experience of a youth's developing intellectually in a different country, particularly one so charged as England with the pursuit of the intellect in the arts and sciences. Consequently I applied for an Athlone Fellowship and a Canadian Commonwealth Fellowship; to my total surprise, I was awarded both but opted for the latter because it was tenable for an extra year.

At this point, without knowing it, my life began to change more profoundly than ever before as, in the short space of four months in 1966, I graduated from UBC and left Canada's shores for an unknown experience in England at the Imperial College of Science and Technology. I had been accepted to pursue my Ph.D. with Professor F. Denys Richardson who, although I was unaware of it then, was one of the greatest metallurgical thermochemists of this century. His influence, together with that of the great historical city of London and the English people, left an indelible imprint on me that remains to this day. Indeed a day does not go by that does not have some reminder of this time. The initiation was hardly ideal because Richardson gave me only a vague notion of what my Ph.D. subject should be; but fortunately he did steer me toward process metallurgy which was in its infancy at that time. He himself was a chemist but, like many scientists working in metallurgy, found the need to embrace engineering as well. Thus part of his vagueness may have sprung from his non-engineering background – his was the vision of a scientist coming to grips with the engineering world. But for all that, he was brilliant with a clear grasp of fundamentals, a sharp analytical mind, and scrupulous honesty in his work. Moreover he was a great communicator in the British tradition of facility in both the written and spoken word. His papers and his presentations are as much an inspiration today as they were when I was first exposed to them. He was not afraid to take the lead, and established both the Nuffield Group in Chemical Metallurgy and the John Percy Group in Process Metallurgy in the Royal School of Mines at Imperial College. By a quirk of fate, I happened to join the Percy Group when the research programs were in full bloom. Sadly, since Richardson's retirement and death, this great effort has faded into oblivion. Timing truly is a major factor in life.

And London with its riches of history, parks, and theatre also cast its spell on the boy from the prairies. Where else in the world could a student afford to see such splendid actors and actresses as Dame Margaret Rutherford, Maggie Smith, Vanessa Redgrave, John Gielgud, Alec Guiness, Tom Courtenay, and Leonard Rossiter live on stage in one of London's timeless theatres? But to see the greatest actor of them all, Sir Laurence Olivier, in two plays – Strindberg's Dance of Death and Shakespeare's Merchant of Venice – was the ultimate thrill. I managed to obtain tickets for the Saturday night performance of these plays by standing in line on the Saturday morning at the Old Vic. Olivier's presence on stage was absolutely riveting, like nothing I have seen before or since. Eyes, voice, body gestures, all drew out each character with such consummate skill – in my mind's eye I can still see him on the stage as if it were yesterday. And when he died, the greatest tributes spoke of his glory as an actor, rooted in courage, romanticism, and optimism.

My research topic was the study of mass transfer between immiscible liquids accompanied by an instantaneous chemical reaction at the liquid-liquid interface. This system is commonly found in pyrometallurgical processes such as steelmaking where sulphur, for example, transfers from the molten metal to an overlying slag (molten oxide) phase. Agitation was to be provided by a stream of ascending gas bubbles which also are common to pyrometallurgical processes. But, rather than make measurements on systems at 1600C, typical of steelmaking, I was to study mass transfer in a low-temperature analogue. The first system I examined followed on from the work of a previous student and involved the transfer of indium (or zinc) from mercury to an aqueous solution of ferric nitrate. In the process of transferring from the metal bath to the aqueous phase, the indium was oxidized from the atomic to the ionic state by the ferric iron. The idea was to measure the rate of indium or zinc transfer under different conditions of mass-transfer control and gas agitation rate, in order to determine mass-transfer coefficients which could be incorporated into dimensionless correlations for scale-up to real process conditions. Also Richardson asked me to look into an observed anomalous increase in the mass-transfer coefficient under conditions where transport of indium in the mercury limited the overall rate of the process. It was pursuit of the latter question which gave me my first taste of excitement of a research discovery. For what I found, with the aid of a schlieren system (borrowed from Prof. Harry Sawistowski in Chemical Engineering), was that, under the conditions in question, the interface between the indium amalgam and the aqueous ferric nitrate solution spontaneously twitched vigorously giving rise to energetic interfacial agitation. It was the interfacial turbulence that accelerated the observed rate of indium transfer in the metal phase. Naturally the cause of this phenomenon was my next objective which led me to the literature and the influence of electrical potential on the interfacial tension of liquid metal – electrolyte systems: the electrocapillary effect. Thus it seemed certain that large gradients of interfacial tension were generated locally because of fluctuations of electrochemical potential caused by the indium/ferric ion reaction in our experiments. The interfacial tension gradients caused the interface to move spontaneously; this movement, in turn, created new fluctuations in potential and ongoing motion. When I investigated the literature further and held discussions with Sawistowski, it became clear that Chemical Engineers had been studying mass transfer and spontaneous interfacial turbulence, driven by concentration gradients, for years in aqueous/organic systems common to their processes; indeed Sawistowski was a leading authority on the subject. Obviously from a practical standpoint, this phenomenon, known widely as the Marangoni Effect, assumes considerable importance owing to its influence on absorption rates and phase separation, and hence on the size/productivity of reaction vessels.

What was perhaps surprising was the total lack of an equivalent body of knowledge on metallurgical systems even though the interfacial tension and the potential for spontaneous interfacial turbulence are considerably greater than in aqueous/organic phases. Later at UBC we would show that the blowing of oxygen from a lance or the deposition of solid pieces of metal sulphide, selenium, or tellurium, all highly surface active, locally onto the surface of a molten metal caused intense spontaneous agitation because of the surface-tension gradients created. One can just imagine the interfacial-tension driven flows in pyrometallurgical processes in which the transfer of surface-active species like oxygen and sulphur frequently play a dominant role.

The paucity of understanding of metals systems behaviour could be attributed in part to the difficulty of making observations at high temperature (1000–1600C) involving opaque liquids. But more important was the neglect of process dynamics by Metallurgical Engineers who had focussed almost exclusively on thermochemistry and characterization of metallurgical systems in terms of thermodynamics. Thus a body of knowledge on process dynamics equivalent to that of Chemical Engineering simply had not been developed in the broadest sense. Richardson had begun to address this failing by launching the Percy Group but it was too isolated from the real world of processing. Although my vision in those neophyte years was not as focussed as it is today, I could see the need and the opportunity to develop the metals processing field.

After studying mass transfer in a second analogue system consisting of molten lead and a fused salt, I wrote my thesis, was married to Margaret Rutter, accepted a faculty position back at UBC, and resigned my commission all in the short space of a few months.

About this time, as the 1960's came to a close, the computer began to emerge as a powerful analytical tool opening up new possibilities for the study of metals processes. Consequently, when I returned to UBC in 1970, I was determined to remove the mantle of the pure experimentalist and to tackle the mathematical analysis of processes with the computer. Soon I was to learn that resulting mathematical models only are really successful if they are based on both theory and experiment. I was fortunate at this time to spend summer leaves of absence at Noranda Research (1971) and Stelco Research (1972 and 1973) where I was exposed to exciting areas of process dynamics. At Noranda, with Nick Themelis and Peter Tarassoff, I worked on submerged gas injection and became actively interested in the copper converter which had been in use for sixty years worldwide without any real understanding, beyond thermodynamics, of its inner workings. At Stelco, I really began to cut my teeth on mathematical modelling with Ray Meadowcroft and Jock McKay as I attempted simulations first of a rotary-kiln process which made iron from iron oxide using coal as a reductant; then I went on to model solidification in ingot and continuous casting. This foray into computer modelling taught me quickly about the need for data (frequently lacking) to characterize properties or process kinetics and to check the validity of model predictions. Sadly this lesson has been lost on many of today's researchers who are more interested in models than they are in processes. I also learned a lesson about a hard reality of Canada; that was, if I wanted to pursue this line of research from a base at UBC, I had to be prepared to fully embrace the twentieth century technologies of travel and telecommunications.

I was fortunate in my early years at UBC to be associated with Fred Weinberg who, at the time, was organizing large-scale trials at operating steel plants to study solidification in the continuous-casting process. He had devoted much of his career to the physical metallurgy of solidification on the laboratory scale and sought to extend his work by investigating what was then a relatively new process for producing semi-finished steel shapes from molten metal. But he needed someone to help him with the mathematical modelling of heat flow in the process and so approached me. The trials had to be organized with meticulous care because they involved the addition of radioactive tracers, in a very small quantity, to the molten steel during the casting process, as well as the subsequent sectioning of the cast product to obtain auto-radiographs. The entire exercise was subject, of course, to Atomic Energy regulations. Results from the trials helped us to validate the mathematical models we developed while at the same time shedding new light on the complex fluid flow and solidification phenomena at work in the process. These trials convinced me even further of the absolute necessity of studying processes, where possible, on the full scale and with the aid of a computer model.

So my original thoughts on process analysis were beginning to come into sharper focus. At this early stage, having been exposed to a number of different processes in copper and steelmaking, I decided to pursue process analysis on a broad front but following the same methodology of combining measurements and modelling. Thus I continued the work initiated at Stelco on modelling the direct reduction of iron ore in a rotary kiln but again came to the realization that surprisingly little was known about this venerable reactor which had been in use since the last century. So, in collaboration with a colleague in Chemical Engineering, Paul Watkinson, a grant was obtained from NSERC, with seed money from Alcan, to build a pilot-scale rotary kiln on which to conduct measurements. We decided to install the kiln in the Metallurgical Engineering building because there was slightly more space; but even this was marginal because there were no laboratories designed for process research. The kiln was made in three sections, the weight of each being dictated by the lift capacity of the freight elevator in the building while the kiln length of 5.5 m was set by the dimensions of the space available on the second floor – hardly sound engineering principles! The unique feature of the pilot kiln was the array of thermocouples we installed to measure the temperature distribution in the refractory wall, freeboard gas, and solids bed as well as sampling ports to determine the extent of chemical reaction of the solids. With the kiln we studied heat-transfer processes and chemical reactions like limestone calcination which led to a series of other joint projects on solids motion during kiln rotation and dust pick-up. Finally the work has led to the development of a sophisticated mathematical model, developed by Peter Barr, which is capable of predicting the performance of operating kilns some of which are over 150 m long. Another fruitful collaboration with Paul Watkinson was the development of a patented fluidized-bed process for roasting molybdenite to replace the conventional multiple-hearth process. A bench-scale pilot plant of the fluidized-bed process was constructed and operated successfully by a very talented graduate student, Igor Wilkomirsky, who is now a professor at the University of Concepcion in Chile.

My brief exposure to submerged gas jets at Noranda convinced me that this was another field well worth pursuing. Although air had been injected through molten copper matte (sulphides) in the Pierce-Smith converter since early in this century, to separate copper from iron and sulphur, little was known about the behaviour of the gas in the liquid. This was not simply an academic question because problems like blockage of the air pipes (tuyeres), which necessitated clearing by insertion of a punching rod, and severe refractory wear in the vicinity of the tuyeres, have persisted over the years. We began to study the mode of gas discharge from a submerged tuyere in the laboratory and found what we were seeking – at the low injection pressure typical of copper converters (about 1 atmosphere), the gas discharges in the form of bubbles so that liquid can contact and freeze against the tuyere between successive bubbles. The resulting accretion formation is the cause of the tuyere blockage. Moreover, by raising the injection pressure, the gas can be made to discharge continuously, preventing liquid from contacting the tuyere and eliminating blockage. To test the concept, my student, Enrique Hoefele, travelled to the Thompson Smelter of Inco and, with a single pipe inserted through the back of a tuyere, injected air without blockage. Later the concept, by then patented with the help of Canadian Liquid Air, was tested on several tuyeres at the Asarco Tacoma Smelter, the Noranda Horne Smelter, and the Inco Copper Cliff Smelter and found to work in each case. To prove the technology finally, a full-scale converter trial is necessary but, to date, industry has been unwilling to commit the necessary funds. Thus injection conditions that prevailed seventy years ago remain today – with the attendant problems. At least now there is an alternative.

An interest in gas injection led me to another process, at Cominco, in which molten slag from the lead blast furnace is treated at 1250°C by injection of pulverized coal with air to recover dissolved zinc oxide. A bright young graduate student, Greg Richards, spent a considerable time at the Trail Smelter probing the secrets of this process which was developed half a century earlier. Samples of slag were taken through numerous fuming cycles; observations were made through the back of a tuyere with a tuyerescope developed in our copper study; and the pressure at the tip of the tuyere was sensed with a pressure transducer to assess gas discharge dynamics. More data was collected from four other smelters world-wide and a mathematical model was developed to simulate the process kinetics. Thus it was discovered that the slag-fuming process could be accelerated to operate at up to twice the rate by injecting a fraction of the coal again at high pressure. Subsequent full-scale plant trials have proved the concept, which now has been patented.

But more important, the fundamental knowledge uncovered in the study is being applied to other injection processes.

Another process which holds particular fascination for me is flash smelting, which was developed originally by Inco in the 1940's. This is an intensive process in which finely ground sulphide concentrates are injected in a jet of oxygen into a furnace where they combust and melt. In collaboration with Greg Richards, I wanted to understand the fundamentals of the process so that it can be designed from first principles to smelt any concentrate with maximum efficiency and minimum dust formation. Thus, with the support of NSERC, Inco, Noranda, Cominco, and Hatch Associates, we have constructed a highly instrumented pilot plant as well as smaller furnaces to study the behaviour of individual particles during oxidation. A mathematical model is in the early stages of development to predict mass and heat flows in the flame as a precursor to a model of the overall furnace.

Of all the processes which I have tackled, perhaps continuous casting has been taken the farthest. Certainly it has taken me a long way over two decades through plant trials across North America, numerous mathematical modelling studies, metallographic investigations, and the development of short courses for industry based on our work on the process. It all began simply by gathering knowledge with Fred Weinberg but inexorably led to pursuit of a host of problems, particularly crack formation and shape defects, that plagued the casting of smaller sections (billets). In addition, our studies have extended to the casting of slabs, horizontal casting, electromagnetic stirring, and the direct-chill casting of zinc. The plant studies were vital to characterize heat extraction by the mould and water sprays and to provide samples of cast product for examination. The mathematical models incorporated the heat-flow information and gave us the fundamental understanding we sought to solve a particular problem. With this approach we solved, for example, a severe internal cracking problem in zinc shapes at Cominco by identifying the source of the tensile stress and then making an inexpensive addition to the spray headers.

Most gratifying, however, has been the work conducted in collaboration with Indira Samarasekera on steel billet casting. Commencing with her own Ph.D. research on mould behaviour, this study has grown in size to include most of Canada's mini steel plants and has resulted in major changes to the design and operation of the mould system with significant improvements to cast product quality. When we first began our research, billet casting was about a decade old and plagued with surface and internal cracks, as well as shape problems, in the cast product. By applying heat flow and stress analyses to the mould, coupled with the plant trials, we were able to elucidate many of the complex mechanisms by which these defects formed. Design and operating changes then were made on the basis of knowledge rather than trial and error.

With this pool of knowledge, one would expect the industry to clamour for change to improve its product quality in the drive to be competitive. One of my greatest frustrations is that the opposite is all too frequently the case, either as a result of sheer conservatism or crushing ignorance. Putting ideas into practice is not easy in such a climate and it has led us to develop short courses on continuous casting for industry as one means of facilitating technology transfer.

When I was an undergraduate, I was struck by the fact that my physical metallurgy courses were largely qualitative in nature, certainly with respect to the application of this knowledge to thermal and thermomechanical processes like heat treatment and hot rolling respectively. This was somewhat curious in an engineering department and was in contrast to many of my extractive metallurgy courses which, at least, made quantitative use of heat and material balances and, of course, thermodynamics. With the passage of time and the concomitant broadening of my view, I came to realize that this was not a local problem but was much more general. Of course, the simple question was why couldn't process engineering tools of models and measurements be applied just as well to solid metal in processes downstream from those we had been studying already. Consequently, in 1985, with the establishment of the Stelco/NSERC Chair and the founding of the Centre for Metallurgical Process Engineering, we launched a program in, what we have termed, microstructural engineering. With faculty colleagues (Bruce Hawbolt, Indira Samarasekera, and Peter Barr), we initiated projects on the reheat furnace, hot rolling, controlled cooling, heat treatment, and continuous annealing. For each process, mathematical models are being developed to predict the thermal and microstructural evolution of steel as a function of steel composition and process parameters. Knowledge of microstructural phenomena which have been treated qualitatively in the past is now being incorporated into the models to make quantitative predictions. The mathematical model, then, is the key to unifying the discipline of metals process engineering. It has been gratifying to see the completion of a user-friendly model of the Stelmor process for the air cooling of steel rod which is capable of predicting the mechanical properties of the rod to within 10%. There is much more to come.

During the first decade of my career, I was occupied also with the development of process engineering courses on heat transfer, mass transfer, and mathematical modelling for graduates and undergraduates. While I enjoyed establishing these courses from scratch and teaching them, I was frustrated by an increased course load resulting from two of our faculty leaving the Department without replacement. Thus, by 1979, I was seriously considering leaving the university when I was awarded an E.W.R. Steacie Memorial Fellowship by NSERC. Although I had received recognition of my research with a number of awards to that point, the Steacie was very special because with the honour came two years of salary support and leave from teaching – time to rekindle my interest. My teaching load was picked up by Indira Samarasekera whose career has since blossomed in its own right. In many respects this was a turning point for me because Stelco also generously offered to fund the Stelco Chair in Process Metallurgy and subsequently the Stelco/NSERC Chair; with this support, my teaching load was permanently reduced so that I could devote more time to research and to outside interests like professional societies.

One of the most satisfying aspects of my work at the university has been interaction with students. There is nothing quite like watching raw intellect take on form and focus and to see creativity flower as a student develops, especially at the graduate level where one-on-one interaction is strongest. However in a few rare cases, inexplicably, this has not happened and, even though the student successfully received the degree sought, I have felt a real sense of failure.

One of my greatest ongoing concerns about students is their ability to communicate in the spoken or written word. It is perhaps my greatest frustration that seemingly bright students have only the most rudimentary writing skills so that a disproportionate amount of my time is spent on their theses or papers to turn a sow's ear into a silk purse – a difficult task indeed! And beyond problems with grammar is the confusion created by poorly organized ideas which are a reflection of muddled thought processes. Students are sometimes surprised when I tell them that they write like they think. Thus I side with those who lament our high school system and our society which, with its emphasis on television and mind-numbing video games, is sinking into illiteracy. There is nothing more beautiful than the written word, polished, expressive, and concise.

Although many of my students (forty have graduated to date) are Canadian, a large number have come from all parts of the globe and have become an international extended family. Consequently, whether my frequent travels take me to Brazil, Venezuela, Australia, Nigeria, China, Japan, India, or France, to name some of the countries, I have good friends to visit. The world really has become a global village.

Although societies are not connected directly with the university, I have enjoyed serving professional societies in both Canada and the United States. As President of the Metallurgical Society (Canadian Institute of Mining and Metallurgy) in 1985, I learned quickly that a certain soft touch is required to lead a society run by volunteers. Interacting with metallurgical engineers across the country, I also came to respect the talent that exists in our professional community whether it be in industry, universities, or government. One of the great endeavours of the Metallurgical Society is the organization of the Conference of Metallurgists which is held each August in a different Canadian city from Vancouver to Halifax. Not only is the Conference a splendid technical meeting, it is a unifying influence in a country of regions. I remember a heated discussion while I was on the Board of Directors of the Metallurgical Society about taking the Conference for the first time to Winnipeg. Several Board members simply wouldn't hear of such a thing because they felt that no one would travel to Winnipeg even to attend our Conference. Fortunately wisdom prevailed and the Conference was sited in Winnipeg. Seven hundred people showed up – a virtual record. What is more, people who travelled to this beautiful city for the first time, were enthralled with the local culture, history, climate, and surroundings. The same can be said for Conferences in Quebec City or Edmonton or Ottawa or Halifax across our great land. We need more of this sort of thing to break down regional barriers and misconceptions - it is central to our survival as a nation.

Particularly in the last decade, I have had more than my share of recognition for my research. It is humbling to be included among the Fellows of the Royal Society of Canada (1987) and Officers of the Order of Canada (1989) as well as to receive the Izaak Walton Killam Memorial Prize for Engineering (1989), the Ernest C. Manning Principal Award (1987), B.C. Science and Engineering Gold Medal (1985), CIM Alcan Award (1988), and Bell Canada Forum Award (1989) amongst others. The struggle and hardwork apparently have not gone unnoticed.

Looking back on my career, I would like to think that it has had another dimension, perhaps political in nature, beyond pure science and engineering. I have come to realize that much of my time has been spent in the gap between solitudes: solitudes created by regionalism in Canada, solitudes arising from cultural differences between universities and industry, solitudes existing between the steel industry and the non-ferrous industry. My fervent hope is that my attempt to bridge these solitudes, with its own set of rewards and frustrations, will strengthen, in some small way, the fabric of the nation.

Looking ahead, for I am only at the midpoint of my career, God willing, I want to continue my research in process engineering but begin work on processes for new materials such as ceramics and composites. I want to continue my direct involvement with young people. I want to fulfill my dream of establishing the best process engineering centre anywhere.

My great hope for the future is that Canada will finally make a commitment to develop her human resources with the same vigour as she has developed her natural resources. We must place much greater emphasis on the development of the human intellect in all sectors of society in order to meet the challenges of the future. The role of education, and research and development, must be strengthened as never before. And the decision makers in government and industry must be convinced that the future of Canada rests entirely on her people. I shall be working toward that vision.

Jules Hardy

(July 16, 1932–   )

Killam Laureate, 1989

Jules Hardy has received international recognition for his original contribution to neurosurgery, particularly for his work on the pituitary gland. Among the pioneers in microsurgery during the early 1960s, he was the first to demonstrate the possibility of identifying and preserving the normal gland during the removal of the large pituitary adenomas. Moreover in patients suffering from hypersecreting pituitary disorders with normal-sized sella turcica, he first described the finding and selective removal of intrapituitary microadenomas and advocated the transsphenoidal microsurgical exploration as the first approach to the therapeutic management of Cushing's disease and acromegaly. Similarly, he first described prolactin microadenomas in the syndrome of amenorrhea-galactorrhea-infertility thus clarifying one of the many causes of hyperprolactinemia. Restoration of fertility after transsphenoidal microsurgical selective adenomectomy in sterile women is considered a major achievement in functional neuroendocrinological surgery. In this outstanding development, he follows the pathways of historical giants who preceded him, Guiot, Dott, and Cushing.

Author of over 100 scientific papers and several chapters in textbooks, his contribution is not limited to the pituitary but extends also to several other topics on basic neurophysiology and functional neurosurgery. His earlier contributions were on microelectrode recording in the thalamus during stereotactic surgery for movement disorders. He further applied the electrophysiological investigation to other structures and he first described the electrical activity of the human neurohypophysis. He also applied this recording method to the spinal cord for selective graded cordotomy by the anterior cervical approach.

Jules Hardy was born the 16th of July, 1932, in Sorel, a small city on the shore of St. Lawrence River and renowned for its marine industries. At the age of 8, he saw his mother succumb to eclampsia. This tragic event must have accounted for his future choice of a career in medicine; although virtually unknown to him, she has nevertheless been omnipresent during all his life. His search to keep her alive is obviously exemplified in the dedication of his last monograph on "Prolactinom" (Neuro-Chirurgie 1981, Volume 27, supplement 1). Accompanying on the front page a beautiful reproduction of a masterpiece painting from Picasso entitled *La Maternidad*, he writes: "For those who have been cured [. . . by selective pituitary adenomectomy] and who gave birth to a child, our therapeutic success is a tribute to motherhood. Through all those women, we honor our beloved mother."

During his years in classical humanities, he was greatly influenced by the Jesuit educators and particularly by the ideas of Theilhard de Chardin, the famous and controversial archeologist, philosopher, and theologian. At this College Brébeuf in Montreal, which Hardy attended, were many elite figures who contributed actively to the deep transformation of Canadian and Quebec society in the 1960s which has been called "The Quiet Revolution". Two of the graduates became Prime Ministers.

As a teenager, he was more concerned with literature than science, and he wrote several hundred pages of poetry. This passion for literature persisted throughout the years at the School of Medicine of the University of Montreal as a collaborator to the Quartier Latin, an intellectual university journal that was a platform for many of the leaders of Canadian politics. While a medical student, he won the first prize in a literary contest held in the Canadian universities and a second prize of the American Christopher Context for a movie scenario. At the same time, he won a prize for a scientific paper, his very first, on "Physiology of the Brain Cortex and Conditioned Reflexes".

After graduating *magna cum laude* with his M.D. degree at the University of Montreal, he trained in neurosurgery under Claude Bertrand, one of the founders of stereotaxic surgery and a leader in the surgical treatment of parkinsonism in the 1950s. He completed his training at the Montreal Neurological Institute where he learned the fundamentals of neurophysiology with Herbert Jasper. At this time, his major interest was oriented toward the study of functional disorders of the nervous system: epilepsy, abnormal movements, and pain.

As a fellow of the McLaughlin Foundation, he had the opportunity to study further stereotaxic surgery in Paris with Gerard Guiot, who was already known for his pioneering work in that field. This was a fortunate encounter; with Guiot and Mrs. Albe-Fessard, they first applied the technique of using microelectrodes to record the human thalamus. This method became a major improvement for the electrophysiologic exploration of the basal ganglia and an invaluable source of knowledge on the basic human neurophysiologic mechanisms.

While mainly working on this method, it was almost by accident that Hardy became involved in the surgery of pituitary tumors. Guiot had just begun to use the transsphenoidal approach he learned from Norman Dott in Edinburgh, the latter having been a pupil of Cushing. Hardy was impressed with the benignity and rapidity of the procedure performed by Guiot and the excellent results with rapid recovery from visual disturbances. With conviction, he returned to Montreal and introduced the method in Canada in 1962.

A major problem remained at that time, however, as was clearly stated by Guiot: although "... the complete removal of a pituitary adenoma might restore the previous hormonal deficit, one should agree that this is rather exceptional ... . From this point of view, one must say that the patient is rather endangered since the adenomatous tissue is *not separable* from the normal hypophyseal tissue and the attempt to perform a complete radical excision brings in the curettes the glandular tissue still functionally active. Therefore, this increases the hormonal deficit, which can lead to a complete pituitary insufficiency following surgery."

This was upsetting to Hardy. When he decided to use the surgical microscope early in his experience, he soon became aware of a new fact: At higher magnification, he was able to identify the remnant of the normal pituitary gland separate from the tumor tissue. As a result, he decided to preserve the pituitary and thus avoid the creation of new deficits. Later on he observed the restoration of functions that were previously lost by tumor compression. On the basis of this experience, he first advocated the early removal of pituitary tumors before the onset of visual disturbances as a purely endocrinological indication. By 1966, he rejected the classical concept of diffuse hyperplasia or hypertrophy of the pituitary gland as he was already convinced that hypersecreting pituitary disorders are caused by the development of a pituitary adenoma. As one of his typical truisms, he contended: "Before becoming big, the adenoma must be small." Like astronomers who predict the finding of a new star only because it should be there as a result of their speculations, this was how the concept of microadenomas originated in his mind before he had the opportunity to find one. This concept was further confirmed in patients with normal-sized sella turcicas in whom the microsurgical exploration allowed him to discover intrapituitary microadenomas. His first paper on the subject was presented at the Congress of Neurological Surgeons in Toronto in 1968. These findings were met with skepticism and controversy for many years by many surgeons and endocrinologists until microadenomas came to be recognized internationally 10 years later at a meeting on pituitary microadenomas in Milan in 1978. He also described a topographical distribution of the secreting microadenomas which will remain a classical finding in pituitary pathology.

Back in Montreal, Hardy was appointed Professor of Clinical Neurophysiology in the Department of Physiology of the University of Montreal where Poirier and Cordeau, renowned neurophysiologists, were actually producing experimental models of tremor in monkeys.

As early as 1962, Hardy published simultaneously the two papers that outlined his major fields of interests: "Transsphenoidal Surgery of Pituitary Tumors" and "Electrophysiological Localisation and Identification of Subcortical Structures as an Aid to Stereotaxic Surgery." It is worth pointing out that 90% of his publications and endeavors are devoted to these subjects: clinical neurophysiology and neuroendocrinology.

One of the less well-known aspects of J. Hardy is his creative, artistic mind and his talent for developing new instruments. His passion for tools was probably inherited from his father, who was the owner of a hardware store in his hometown as well as a painter and craftsman. Although Hardy is not recognized as an expert in any particular form of art, he nonetheless adds an artistic touch to every one of his works. This brings him particularly close to those scientists whose imaginations dominate technology. If he had failed to feel the passion to repair—like the old watchmaker— he would probably have become a science-fiction writer or a sculptor. And he has made some valuable artistic creations to help others understand the complexity of the brain.

He designed a cartesian multicolored Plexiglas model of the human thalamus to help localize the thalamic nuclei precisely and safely during stereotaxic surgery. He also designed the gigantic tridimensional model of the human brain exhibited in the "Man and His Life" pavilion at the International Expo '67 in Montreal. In this 16-foot high model, thousands of computer-controlled light bulbs lit up in sequences with a movie that demonstrated the major functions of the brain. This ingenious display was a precursor to the imaging process of positron emitting tomography, and preceded it by 10 years. With Hardy's creation, however, the picture was formed much like an impressionist painting: When observed from a close distance, one saw only spots of colors and shadows. Like those of the early impressionist artists, some of his early creations were lost before anyone—himself included—realized their value.

Hardy is also a master in the art of finding the concise image that says everything without a word, for example, his famous "pimento in the olive", representing the microadenoma in the pituitary. The same can be said about his operative protocols, which are dictated personally. Each one is different from the other, reporting not only the operative findings, but also the feelings, joys, fears, and deceptions of the surgeon; each one is like a milestone marking another step towards perfection. His protocols are often accompanied with a multicolor sketch drawn in the coffee-shop after the operation.

His talent with handicraft is shown in the numerous surgical instruments he has devised; the microelectrodes and microdrivers are still used for stereotaxic exploration of the thalamus, the spinal cord, and the pituitary, the renowned set of instruments for transsphenoidal pituitary surgery which is widely used around the world.

Hardy possesses a strong character and a legendary capacity to provoke and face challenge. In a letter of recommendation for his new appointment as a young surgeon in Montreal, Gerard Guiot wrote about him: "Hardy does not always have a good character but, at least, this is certainly the proof that he has some." During a general strike of the hospital employees throughout the Province of Quebec, he published in the newspapers a strong letter addressed to the president of the labor union condemning his lack of moral conscience and another to the Prime Minister of Quebec condemning his lack of authority. The reaction from prominent members of Parliament and the syndicates in a combination of insults and personal threats by no means prevented him from denouncing whatever injustice appears to be worthy of challenge, no matter who is the author—even at the highest level of the government.

Hardy is a highly controversial figure in his milieu because he has often taken firm stands alone and without seeking general agreement by his colleagues. On the rare occasions of public appearance, he has frequently shocked the audience with statements against the establishment. In this way, he purposely deterred administrative promotions that would otherwise impede his scientific career—a sort of voluntary creative incompetence according to the Peter Principle.

This attitude contrasts neatly with his unfailing fidelity towards his friends, although never as a negative critic or with the slightest shadow of jealousy. Any new idea, suggestion, or even criticism coming from a friend will be received by him with open-mindedness and astonishment. I do not remember Hardy delaying, shortening, or cancelling an impromptu meeting with a colleague or a close collaborator. On the other hand, he would consciously omit attending a group meeting or committee because he does not believe in creation by institutional groups or collective thinking. For him, creativity in every field—whether in the arts or the sciences—is always a personal endeavor. Like a mountain climber or an explorer of the wilderness, he avoids the well-known paths, and he is happier in the middle of the unknown or the unexpected.

A natural leader, he presided unchallenged over an important group of investigators from different institutions, both in basic research and clinical work, without any official structure or hierarchy. In this period of history when the responsibilities for the advancement of sciences are shifting dangerously towards governmental control, it is worth pointing out that practically all the scientific achievements of Hardy and his collaborators were done "without any official financial support", as was explicitly written in a footnote of one of his early publications.

In the pursuit of his intuitions, he demonstrated an unshaken enthusiasm. A permanent inspiration for his collaborators, he always pushes them to give their best, and nothing shocks him more than a lack of enthusiasm or mediocrity. In many fields, he has promoted younger fellows. For example, the development of microvascular surgery in Montreal, the use of the laser for the treatment of aneurysms, and the studies on the vascular anatomy of the hypophysis were all results of his initiative, yet he gave all credit and recognition to the fellows and assistants to help promote them.

Now, 20 years after he began practicing neurosurgery and 15 years since the historical surgical removal of the first pituitary microadenoma, he has accomplished a gigantic task. His experience extends to over 1500 transsphenoidal operations. He has personally taught transsphenoidal surgery at home and abroad to neurosurgeons from the five continents. World-wide recognition for his work brings to him honorary distinctions from universities and governments of many countries. The last honor he received was the appointment by the Royal College of Surgeons of England as the Sir Arthur Sims Commonwealth Travelling Professor for 1983. He is recognized for his merit all over the world and at home in Montreal, where he is presently the Chairman of the Division of Neurosurgery at the University of Montreal; it is my feeling that this will provoke no more concern to him than the controversies and challenges of the past years. If he has acquired a lot, he has given a lot and to him apply the sentences of Jonathan Livingston Seagull:

> " It's good to be a seeker
> But sooner or later
> You have to be a finder.
> And then it is well
> To give what you have found
> A gift into the world
> For whoever will accept it."

Pedro Molina-Negro, M.D.[1]
Montreal, Quebec, Canada

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J. TUZO WILSON

(October 24, 1908 –   )

Killam Laureate, 1989

1. Youth and introduction to science

My mother and father met in 1906 when each was on a holiday at Banff, Alberta. Dad was a Scot, a vigorous man, who had played amateur rugby for Yorkshire before practising engineering in India and then in Canada. During World War I, he became the Assistant Deputy Minister in the Naval Department and in 1918 he undertook his life's work, overseeing the development of civil aviation for Canada. The visitors he brought to our home introduced me, at an early age, to the excitement and opportunities of flying and exploration.

Mother was a Canadian, born on Vancouver Island. Her father, Dr. Henry Tuzo, was of Huguenot-American ancestry as his name, once Tuzeau, illustrates. In 1853, upon graduating from McGill, he had joined the Hudson's Bay Company as a doctor and crossed an untamed and empty Canada with the fur brigade under Sir George Simpson. Soon he abandoned medicine for banking, and Victoria for Wall Street. After making a small fortune, he retired to Surrey where he died. My mother often took my widowed grandmother travelling and, while at Banff in 1906, she made the first ascent of the last unclimbed mountain in the Valley of the Ten Peaks at Moraine Lake. It was later named Mount Tuzo, and is the most prominent of the peaks illustrated on the back of the Canadian twenty-dollar bill.

The following year my parents married and settled in Ottawa, in a romantic but ramshackle house, where I grew up. I just remember meeting the previous owner, a charming old gentleman, Canon Georges Bouillon of Rimouski, one of Canada's most distinguished architects. He used the house as a summer cottage and workshop for his experiments in church architecture. His greatest achievement was a convent chapel, with a glorious ceiling of carved wood, now a showplace in the National Gallery in Ottawa.

That house, with its striking appearance and a splendid view, was set upon two acres of hillside at the edge of the woods and pastures which then constituted Rockcliffe. Its drawbacks were that the house was miles from anywhere, was cold in winter, and the garden required much work.

Just before I was five, we went to England and returned with a young friend of mother's, who, for the next two or three years, spent the mornings in teaching my sister, Letty, and me most of the primary school curriculum. In 1915 the first coeducational school opened in Rockcliffe. It is now Elmwood School, but then was accommodated in one room of an old house to which we walked a mile and back again twice a day over muddy roads. Apart from passing three solitary houses and a pond, our route lay through pastures inhabited by alarming cows, an area now an exclusive haunt of diplomats. Our early teaching at home meant that in school we were both younger and smaller than our classmates, a discrepancy that tended toward an independent cast of mind and a poor opinion of team sports.

My first recollection of science is of a visit to the National Museum of Canada. Dr. Prince, a curator, showed us a whale skeleton, a large Haida dug-out canoe, and much else. The date must have been prior to February 3rd, 1916, because that evening, from our bedroom windows, we watched the Parliament Buildings burn and the next day the MPs took over the Museum for their temporary meeting place.

I was also entranced by the Ottawa Field Naturalist's excursions, an interest that was intensified in 1916 when Diamond Jenness returned from the 1913-1918 Canadian Arctic Expedition led by Stefansson. The only person he knew in Ottawa was my father; so we saw much of him. He took me canoeing and told stories of his life among the stone-age Eskimo. Later, I met Stef and many other Arctic explorers.

I also knew C.P. Edwards, who had been a radio telegraph operator with Marconi in Newfoundland. He continued to experiment and since, at that time, no radio stations were operating regularly, he persuaded me to act as a substitute for a broadcasting station. In return for winding up a mechanical gramophone and chattering into a microphone, he invited me to the Chateau Laurier when he gave the first public demonstration of radio reception by voice from Montreal.

After Elmwood, I went to Ashbury College and Lisgar Collegiate. Both taught me well and in 1926 I enrolled in Honours Maths and Physics at the University of Toronto.

2. Life at universities and problems in geology

Physics, chemistry and astronomy were all well taught but were presented from totally different points of view.

Professor Lash Miller, head of the Department of Chemistry had taken his Ph.D. in Germany before the discovery of natural radioactivity, and continued to lecture with great panache on a chemistry based upon a precise thermodynamic theory, airily dismissing the impassioned views held by Professor John McLennan, head of the Physics Department in the building right next door, on the new physics introduced by the discovery of X-rays, radioactivity, and the theories of relativity He maintained that all these new-fangled concepts were irrelevant to the study of chemistry and went so far as to threaten to fail any student who mentioned electrons or molecules.

The majority of the Physics Department understood little more than Lash Miller of the mysteries of the atom and confined themselves to teaching the precise, simple, useful laws of classical physics that governed Newton's clockwork universe. These laws are as valid and as useful as ever they were and have greatly influenced my thinking.

As for astronomy, in those days before distant galaxies had been discovered, it was, like chemistry, largely a practical subject, since the stars in our galaxy provided the only accurate means of telling time or of navigating.

Although the courses were demanding, they were not exciting. I mastered them, but two gnawing objections remained. One was that I was vaguely puzzled at the compartmentalization of the sciences and why it was dangerous to mention, from one department to the next, what had been taught. The second, upon which my views were much clearer, was the extreme tedium of conducting experiments, for hours on end, in hot and airless laboratories, with primitive equipment, before the days of electronics and nuclear science. There were no computers, no display screens, no mass spectrometers, no modern equipment. There was no television, little radio broadcasting, and automobiles were few.

In contrast, the summers I spent in field work were stimulating and rich in variety. I started as a schoolboy in 1924 and continued for fourteen summers. In 1927 I looked for gold north of Lake Superior with Noel Odell, a fine geologist who had been the hero of the 1924 Mount Everest expedition. The baffling intricacy of Precambrian rocks, studied amid the joys of outdoor life, promised a much more fascinating life than the study of classical physics in antiquated laboratories. In 1927, although I had not taken a single lecture in geology, I went to my professors and asked for a transfer to that subject. The entire faculty was struck dumb with shock, except for Professor Lachlan Gilchrist, as shrewd as his Orkney forebears, who pointed out that geophysics had been, by several years, the first subject to be studied on the land destined to become the future University of Toronto. Following the discovery in 1831 of the north magnetic pole in the Canadian Arctic Islands, three great scientists, Carl Frederick Gauss, Count Alexander von Humboldt, and Captain P.A. Sabine, an Arctic explorer and scientist, who later became President of the Royal Society of London, requested that, to improve the study of the Earth's magnetic field, magnetic observatories should be built in Tasmania, Capetown, St. Helena, and Canada. In 1839, one was opened in a log cabin near the site of Convocation Hall in Toronto. Professor Gilchrist added that, in 1921, the discovery of a geophysical method of finding petroleum-bearing structures had revolutionised the oil industry, temporarily reducing the price of a barrel in Oklahoma from four dollars and fifty cents to fifteen cents; this had created an unfulfilled demand for students who knew something about both physics and geology.

Thus, I was allowed to take an ill-assorted mixture of geology and classical physics. It was just at that time that astronomers discovered distant galaxies, and the development of quantum mechanics introduced nuclear physics. The whole scientific community was galvanized and all three physical sciences united . Nobel prizes began to rain down upon physicists like fragments of rock after a dynamite blast in a quarry, but I was allowed to wander off into the woods, where no one was any wiser than before about how the Earth behaved. The practice of geology was then confined to examining rocky outcrops that poked through the moss and soil, and to recording the details on color-coded maps. As one blundered through the bush with a hammer, it was easier to identify the particular type of rock in an outcrop than to recognize a fault that might stretch for miles. It was not until air photographs came into general use in the thirties that geologists were able to view their country on a grand scale.

Slowly, it dawned on me as I studied the rapid advance of physics, that there must be ways to improve the study of geology.

Thirty years later, that hope was stimulated in a vague way by the discovery of DNA molecules, the double helix, and the genetic code which added modern biochemistry to classical botany and zoology. Thus, I witnessed the enormous additions which two scientific revolutions made to different groups of sciences.

The very success and vast economic importance of geological studies, which are all made at or near the surface of the Earth, have made it more difficult to realise that no one is likely to discover the cause of the Earth's behavior until its whole interior is better known. Unfortunately, methods of obtaining that information in any detail have only been developed within the last five years. Thus, it is only now that the prospect opens that a similarly dramatic and powerful revolution may be underway in the Earth Sciences.

In 1930, when I went on to Cambridge, I did not understand this, but I had the good fortune to take lectures from Sir Harold Jeffreys, who had done more than anyone else to use earthquake waves to explore the Earth's interior. Unfortunately, he was a mathematician and my grasp of mathematics was limited; I understood only a fraction of what he said. Teddy Bullard, who had been slated to teach me experimental geophysics, had been delayed by floods and lions in East Africa where he had been trying to measure gravity in the great rift valleys.

My tutor was James Wordie. He had been to the Antarctic with Shackleton and spent a winter there living on rather raw sealmeat under an overturned lifeboat. Later, he had explored the fiords of Baffin Island, and his tales excited my interest in the Arctic.

My supervisor was John Cockcroft in the very year in which he and Walton won the Nobel prize for first splitting the atom. I met J.J. Thompson, who had discovered the electron, and had retired from physics to become Master of Trinity College, Cambridge, where he tried to teach me lawn bowling. I also met Ernest Rutherford, whose reputation was then at its height, and many other great physicists.

I knew Louis Leakey, who had returned to Cambridge from his native Kenya to study the early human fossils he had collected. Later, I met Joseph Needham, whose monumental work on the history of science in China was to be an inspiration to me at the Ontario Science Centre.

I moved on to Princeton for my doctorate because Professor R.M. Field had plans to start the teaching of geophysics and the physical and geological study of ocean floors. No other universities in the eastern United States had thought of that and his colleagues laughed at his impossible notions, but Field had a vision of extending the methods of seismic prospecting from coastal areas into ocean basins. He had planned to appoint Maurice Ewing, a lanky Texas physicist, of the greatest ability and drive, to do this, but by the time I reached Princeton the rest of the faculty had already appointed another very brilliant young man, Harry Hess, who was, at that stage in his career, an orthodox petrologist. Nevertheless, Field obtained two thousand dollars with which Ewing began his exploration of the world's ocean floors at Lehigh University. D.C. Skeels and I drove over on a few weekends to assist him, but the miserable state of the roads and cars and exceptionally heavy snow handicapped our efforts. Ewing once told me that he regarded the pair of us as his first honorary graduate students. Later, at Columbia, he did contribute more than any other individual to the study of ocean floors.

My thesis was to map the structural geology of a great fault across the Beartooth Mountains of Montana. Hence, in 1934 and 1935, I visited the West during the dust bowl period and at the height of the depression, when several gangs of desperados, including Dillinger and Bonnie and Clyde, were daily shooting up the banks and gas stations in Iowa even as we drove across that state.

I vastly enjoyed two summers of constant climbing and scrambling in the mountain country which was still wild and empty. More or less by accident, all alone and without any special equipment, in September, 1935, I made the first ascent of Mount Hague some twelve thousand, three hundred feet high. Being in very good physical shape, I was able, late in the afternoon of the second day of jogging and scrambling, to reach the top of one of the eighty highest peaks of the United States, until then unclimbed. When I reached the top, I was astonished to find the summit flat, about the size of a football field, and only gently tilted. On every side, except the one up which I had scrambled, high cliffs fell away to deep dark glacial valleys. Having no jacket and limited food, suddenly I felt chilled and hungry. I got down as fast as I could, slept under a tree, swam the cold and rapid Stillwater River, and got back to the ranch. Pondering on the climb later, I realised that, to preserve its flat top, the mountain must have been recently uplifted vertically and not squeezed up like toothpaste some sixty million years ago, which was then the conventional view. This strengthened my belief that geology was rife for change.

3. The geological survey of Canada and the Canadian Arctic

My training in physics and my field experience convinced me of the tremendous possibilities in geology; so I was glad to join the Geological Survey of Canada, whose able director, W.H. Collins, had embarked upon several badly needed modern improvements. The survey had begun to use aerial photographs to make topographic maps instead of making all surveys on the ground and he was pensioning off the copper-plate engravers who had cut the metal plates used to print the maps.

He had engaged a chemist, H.E. Ellsworth, who made early and excellent chemical determinations of the age of uranium minerals. He wanted a geophysicist, but was not allowed to employ one; so he hired me as a field geologist. Although Dr. Collins was rightly held in high regard, his thinking about geology was still confused and provides an example of the difficulty of uniting the sciences. In 1933, I was helping him by measuring the density of rocks which he had collected at Sudbury. There, two basins of igneous rocks lie one within the other. He maintained that a single rock melt had been intruded and that the two basins had separated one from the other while still in a molten state, like oil from water. I hinted that some chemists held that to be impossible. He promptly replied that geology was just as good a subject as chemistry and that the chemists would have to change their minds. It was another example of the concept, which I had seen in 1926, that each discipline was a separate kingdom, but meanwhile chemistry, physics, and astronomy had been united and it puzzled me that geologists refused to recognize the main stream of science.

My first field work was, with the aid of three students, to complete a geological map of a considerable area in Nova Scotia. The finished portion of the map was the product of ground surveys by my predecessors, with additions by our party, greatly speeded by our examination of a set of aerial photographs of the area. I considered them to be a useful tool that enhanced the precision and efficiency of mapping; however, when I submitted the result of my summer's work the future Chief Geologist told me that the use of photographs was cheating because I had not visited every rock on the ground. Nevertheless, the Survey published the maps.

The next summer I spent in and around the mines in northern Quebec, but, in 1938, I was dispatched to the Northwest Territories. To reach the area, our party travelled part of the way down the Mackenzie River system on a wood-burning paddle-wheel steamer. Once there, near Fort Smith, five student assistants and I made a reconnaissance map of nearly five thousand square miles. We travelled in pairs, each of which had to map about fifteen square miles a day. Without the use of air photographs it would have been impossible, but the Editor raised objections to their use and refused to publish much of the information I had gained from them. He, of course, had never had the chance to use such modern aids in the field.

A fascinating feature of that summer was that, in 1771, Samuel Hearne had crossed the region on his return south from discovering the north coast of North America. I discussed his route with two great geologists, J.B. Tyrrell and Charles Camsel, who had already mapped parts of his journey and I was thus able to locate other parts. Dr. Tyrrell told me he was puzzled why Hearne's wonderfully lucid and detailed report, which had at first been so well received, had later been discredited, but he gave me some clues. During World War II, I went to the British Museum, where I believe I found an explanation in the evidence contained in Hearne's original map and in other contemporary documents. Later, explorers had confused Hearne's Great Slave Lake with Lake Athabaska, thus making a nonsense of Hearne's text.

In all those summers I saw much game, mountain sheep, black bears, moose, deer, and cariboo, but everywhere their distribution was sparse. We took a keen interest in the wild life; none of the animals ever threatened us and we never carried the unnecessary burden of a gun. We saw lots of birds' nests and climbed up trees and cliffs to look into the nests of bald-headed eagles. On several occasions we paddled up to moose or cariboo when they were swimming in lakes and grabbed hold of them. Hearne said that it was safe to do this with moose, but not with deer or cariboo. Having found that he was right, I once jumped out of a canoe onto the back of a moose which was swimming in the middle of a large lake. Once, when hungry and again guided by Hearne's account, I killed another swimming moose by leaning out of a canoe and hitting it on the head with the back of an axe. Since the moose was as large as a thin horse, we had a rather difficult job getting it to shore. There was evidence that wolves had begun to attack it, for we saw one run away, but it was we who first profitted from the hunt.

During the winters we all did a routine job of compiling maps and writing notes. On reconnaissance surveys, no long reports were required; so I was able to turn my attention, in company with other young Turks in the geological community, notably, A.W. Jolliffe and G.H. Norman, to the use of air photographs to speed up some types of geological work. Large faults in the rocks and large glacial features left by the last ice age were more easily identified on an air photograph than when standing right on top of them on the ground. Indeed, several of us, in different parts of Canada, by fusing our efforts, were able to publish the first maps of the structural features and of the glacial geology of all Canada. The latter revealed a fascinating picture of the direction of flow of the last ice sheets and of the prodigious temporary streams which had carried away the melt water.

4. Marriage and the army

Living in Ottawa, I was rewarded by seeing Isabel more often; shortly before World War II, we were married. The depression, lack of jobs, and the customs of the times had delayed many marriages, and at all the universities I attended, nearly all of the students were single.

I was in the Canadian Military Engineers from 1939 to 1948, almost seven years, of which I spent over half overseas. Isabel made her own way overseas and joined me in London for most of that time.

I had joined up as a field engineer, gone overseas in a survey company, and was then transferred to become a geologist and second in command of the First Canadian Tunnelling Company. This name was a cover, because the object of the unit was not to drive tunnels but to drill horizontal holes in which to place explosives. In the spring of 1940, we stopped preparing to blow up the Siegfried Line and assumed an active part in preparing the defence of Britain against possible invasion by laying charges which could be detonated to blow up roads and air-fields, a stratagem most useful near the coast. Thus, from Dover Castle, I watched Stuka planes dive bombing convoys in the Straits, and later from fields around Canterbury saw fleets of as many as fifty-four German daylight bombers headed for London only to be scattered and turned back during the Battle of Britain.

The army then transferred me to a job investigating technical developments in the elaborate preparations for the assault on Europe. In the course of this, I was sent to the Mediterranean on an inspection tour of the activities of engineers there. I went as an unofficial A.D.C. and report writer with the Deputy Engineer-in-chief of the British Army. After almost four years overseas, I was ordered home. Isabel and I returned to Canada on the Queen Elizabeth. We had a rough winter passage while the unescorted ship zig-zagged her way across the stormy North Atlantic at her full speed of thirty knots. Appointed the first Director of Army Operational Research, I soon discovered that no one had any clear idea of what I should do. Thus, the job was a carte blanche to use my staff to do whatever others requested or whatever we thought needed doing.

5. Exercise Musk Ox and the opening of the Canadian Arctic

Our most novel enterprise was Exercise Musk Ox, one which we dreamed up in 1945 when the war was drawing to a close. Col. Wally Goforth, a brilliant economist and odd job man for the Chiefs of Staff, had asked me what the army could most usefully do with all the winter warfare equipment that had been built in secret at Winston Churchill's suggestion. The Allied Chiefs of Staff had realised that cold weather had defeated many armies, including those of Napoleon and Hitler. They had requested Canada to undertake secret preparations for winter warfare. The army, by great good fortune, had enlisted the help of Armand Bombardier, a Quebec garage owner and mechanical genius, who had discovered the basic requirements for successful over-snow and all-terrain vehicles. With the army's help, he had built the large and well protected forerunners of the first snowmobiles. These, with R.C.A.F.'s new methods of providing air supply, revolutionised winter travel in the Arctic, which until that time had depended on dog teams.

All the equipment needed for successful winter warfare had been built and tested, partly under the supervision of Lt. Col. Patrick D. Baird, who had been brought back from his job of training commandos in Scotland. As one of the seven members of the British Canadian Arctic Expedition of 1935-1939, he knew the ways of the Arctic. The members of that small expedition had funded themselves and joined the services without writing an account. Hence, it is little known that the participants, while living like impoverished Eskimos, had done much scientific work. They also discovered and mapped several large islands in Foxe Basin just north of Hudson Bay, probably the last new land to be discovered anywhere in the world. Unlike most Polar expeditions, the B.C.A.E. had achieved its objectives with great success and no publicity.

Pat Baird and I decided that the answer to Col. Goforth's question would be, as soon as the war was over, to mount an expedition to demonstrate Canadian ability to travel in our own Arctic. We recruited others who knew how to operate in the Arctic. It would be a scientific expedition and well publicised. The final plan was for ten vehicles carrying forty men to start from Churchill on Hudson Bay in February, 1946, and travel north to the Arctic Islands, west to Great Bear Lake, and then south to Edmonton. Baird led the main party which, in eighty-one days, with superb support by the R.C.A.F., covered over three thousand miles without roads, on schedule. It arrived only one day late, delayed near the finish by early breakup of the ice on the Nelson River. The leaders included several men with much Arctic experience with the R.C.A.F., Canadian Army Signal Corps, and mining companies.

The R.C.A.F. had asked for an advance airfield on the ice of Baker Lake in the heart of the Barren Grounds west of Hudson Bay. Tractors had to be driven six hundred miles, through little known country, at the darkest and coldest time of the year. Lt. Col. Graham Rowley was called back from Europe to lead the Advance Party. Like Baird and Lt. Cmdr. Thomas Manning and his Nova Scotian wife, Jacki, who were advising us, Rowley had also been a member of the B.C.A.E. I went to Baker Lake by the second plane ever to land there in winter. It was fifty degrees below zero and was soon blowing a blizzard which obliterated the view and tended to freeze one's eyes shut. The only permanent inhabitants were seven whites in tiny houses and six infirm Inuit in igloos. The lives of the Inuit had not changed greatly for centuries.

A few weeks later, the arrival of the snowmobiles at Coppermine impressed upon me how great would be the impact of snowmobiles and winter flying on life in the Arctic. That is what we had planned to demonstrate. Tom Manning and I had arrived by air a day or two ahead of the convoy, and after borrowing a dog team, had driven thirty miles offshore to visit an Inuit village of a dozen igloos. The natives gave us dried fish to eat and the meat of seals which they were spearing through holes in the ice. We watched a traditional drum dance in one large igloo, then retired to another, where we slept in furs on a snow bench, the other end of which was occupied by an Eskimo family. The wife tended the moss wick burning melted seal blubber in a soapstone lamp.

We returned in time to meet the first ten mechanical vehicles ever to reach Coppermine. They rolled in, exactly on time, and next day I drove one of them across the open but rocky barrens, past Hearne's Bloody Falls, across the Dismal Lakes, and over the rocky hills and snow to the uranium mine on Great Bear Lake.

We had invited the Americans and the British to send observers on Exercise Musk Ox and four American officers, well seasoned in war but ignorant of the Arctic, and one British officer, experienced in both, had accompanied the thirty-five Canadians, several of whom had lived in the far north. Thus, for once a polar expedition set out, led by experienced men who knew what they were doing, who had designed their own plan, timetable, and objectives. Apart from the R.C.A.F.'s American DC-3 planes and gliders, the equipment was Canadian. During the war there had been great activity in the sub-Arctic, especially on routes to Alaska and to Europe, but little attention had been paid to the true Arctic, that is, to the Barren Grounds and Arctic Islands in high latitudes near the pole. When the war was over, the Americans began to concern themselves with the high Arctic. They installed primitive Loran stations in Canada to aid navigation and began flying long-range flights from bases built during the war in Alaska and Canada. On their first flight to the North Pole, the U.S.A.F. invited Air Commodore Keith Greenaway and myself to go as observers. The flight lasted twenty-two hours non-stop and went from Fairbanks, Alaska, to the Pole and back to Edmonton. The object of the mission was to search with radar for more islands but none were found; so Keith and I not only became the second Canadians ever to see the North Pole, but we also flew in the final search for new land. The last of the unknown islands in the world had been discovered in January, 1941, by the Mannings on their honeymoon, which they had paid for themselves. These, the Tweedsmuir Islands, lie off the west coast of Baffin Island.

6. Professor of geophysics and world traveller

In June, 1946, I succeeded Professor Gilchrist as Professor of Geophysics in the Physics Department at the University of Toronto. Since I had been unable to get a Ph.D. in geophysics in the 1930's, my degree was in structural geology, but I was, nevertheless, given tenure as a full professor in the Physics Department.

Gilchrist, with his usual foresight, had already assembled a staff in geophysics of four able professors and lecturers. Until the end of the war, there had been few students, but now they arrived with a rush. Their interests had centred on the application of geophysics to the search for minerals and petroleum, but, at that time, Norman Keevil was also building a mass spectrometer to make precise determination of the age of rocks. After he left to prospect with great success, other physicists, led for a time by Professor Teddy Bullard, completed it. I turned to what I knew best, elementary physics applied to the Earth as a whole and the development of ways to speed up those studies. From the start, these included air photographs and new geomagnetic methods developed during the war to search out enemy submarines. These airborne geophysical techniques were expanded later into remote sensing from satellites and all of these methods have had a far larger effect upon Earth science than any of us then realised could be the case. As a corollary to my use of methods which could map large areas, I became active in compilation of existing data over large areas.

My connections with Ottawa were invaluable. As the war drew to a close, Dr. C.J. Mackenzie, the able and hard-driving President of the National Research Council, with the backing of the Hon. C.D. Howe, was reorganizing the support of science. He modified old committees and created new ones, providing each one with travelling expenses and a secretary. Later, he organized good systems of making grants to support research.

He gave me carte blanche to coordinate Canadian efforts in geophysics and, in 1948, to lead several Canadians to the first post-war meeting of international geophysicists. Because of the pioneering efforts of Gilchrist and of Canadian meteorologists, oceanographers, and a few other specialists, we were well qualified and several of us were elected members of international committees. The meeting over, I went on to a similar meeting of world geologists in London and a two-week field trip in the Scottish Highlands.

Canadians became very active in geophysics and, according to an American survey, at one time, a quarter of all geophysical prospecting for minerals, apart from petroleum, was being conducted by Canadians.

In 1950, Sir Mark Oliphant, who was establishing the science side of the newly formed Australian National University, invited me to come for three months and report on what I felt the university should aim to do in geophysics. I was guided through every state in Australia by local geologists, who explained the regional geology. The Carnegie Foundation topped it off by providing me with a side trip to South Africa and a stopover in India.

No one could have been more hospitable than the Australians, especially Robert Thyer, Jack Raymond, and Reg Sprigg. I met the legendary Sam Carey, who became a good friend and who chose, when sixty-five, to arrive by parachute to give his final lecture at the University of Tasmania. He was an ardent advocate of continental drift, but in 1950 my mind was closed to so outlandish a concept, although later, his efforts did much to convert me.

In Adelaide, the famous Australian Antarctic explorer, Sir Douglas Mawson, took me on a field trip and asked me if I would be willing to succeed him in the chair at Adelaide.

Professor Kerr Grant told me how he had collected enough observations of a very bright shooting star to establish approximately where it had fallen. He had gone with students and found a broken stoney meteorite lying on the desert in the small impact crater it had made.

In the gold fields of Kalgoorlie and in the lead and zinc mines at Broken Hill, I went underground. I swam on the Great Barrier Reef and on beaches on the Pacific Coast. Bob Thyer took me through the hot dry outback where I met Dr. Flynn, the Flying Doctor, and saw the very beginnings of the uranium mining at Rum Jungle, a region then so remote that a prospector was shacked up there with two native women in a shelter that consisted merely of a roof and screening. It is so hot near Darwin that even the termites built their ant hills in the shape of great flat sails, facing east and west so that they presented only an edge to the noonday sun, thus, exposing the smallest surface to the full tropical heat.

I had reached Australia by taking a plane to London. It lost an engine over the Atlantic. From London a flying boat took me across Europe and up the Nile to reach Johannesburg in four and one-half days, stopping every night on the Nile or on great lakes. In South Africa the geologists had long felt isolated and made much of every visitor. Very generously, they flew or drove me all over South Africa; there, among others, I met R. Broom, an early expert on primitive man. Later, and elsewhere, I was to meet others who had found Peking man and Java man.

In South Africa, all the geologists were disciples of Alfred Wegener and A.L. du Toit, and they were anxious to correct my failure to accept continental drift, but I remained inflexible for another nine years. At all the three universities which I had attended, this concept had been so strongly condemned and so little discussed that I had never examined it dispassionately. It is indeed true that science can be tested in impartial ways, but it is also true that scientists habitually avoid any dispassionate examinations which might cause them to change their minds.

The South African geologists took me to diamond, gold, and chromite mines, to the great Bushveldt igneous intrusive, and to possible meteoric impact crators. They flew me down the Orange River, south along the Atlantic, and over the well guarded Skeleton Coast with its diamond-studded beaches. I saw the superb wild animal preserves, the beauty of the land, and the division of its people.

In India, I stopped long enough to give a talk, in sweltering heat, in Calcutta, to the Geological Survey and another at Presidency College; then I went on to Singapore, in those days heaving with revolution. It was still a shabby Malaysian village; the only buildings of any consequence were the white colonial headquarters grouped around a square, and there was little inducement to delay my flight to Australia.

My three months in Australia over, I flew north to New Guinea, past great mountains that rise to over eighteen thousand feet and over tremendous gorges sliced through the jungle until we reached the very centre of the island where the Australians had only recently penetrated. I was travelling in a gypsy moth biplane with a landing speed of only about forty miles an hour which the pilot said suited him admirably because, if forced to make a landing on the top of the forest canopy as he expected he might someday have to do, he hoped to survive to climb down a tree which, as he said, he would not have climbed up. The forests were full of cockatoos and in the mountains the cheerful natives were still wearing little except strings of banana leaves, house paint on their faces, sticks through their noses, and birds of paradise feathers in their hair.

We flew on to Rabaul, on the island of New Britain, passing numerous active, but not violent, volcanoes puffing steam, and landed in the harbor. It is the breached crater of a moderately quiescent volcano, on the inner shores of which I stayed in a metal shed built by the Americans in World War II and since converted into what passed for a hotel. The local people had very vivid memories of James Michener who, only a few years before, had lived there when he was writing Tales of the South Pacific.

George Taylor, the local government volcanologist, led me up one of the small volcanoes surrounding the harbor, which had erupted from the sea within living memory. Nearby, the sea-water was hot and steaming from volcanic vents below, but not far off were wonderful coral reefs teeming with life. On our return journey, we passed Mount Lammington, a conspicuous and allegedly extinct volcano, but a few months later it blew its top and buried seven thousand of the unfortunate local inhabitants. Taylor received the George Cross for his efforts to rescue survivors.

I flew back to Canada via Fiji, Canton Island, and Hawaii in one of the early planes on the Pacific route. In Hawaii, what few tourists there were came by ship, and the only two hotels on Waikiki beach were not crowded. In the heat of the day we saw only six people on the beach. Later, adding up all the side trips, I found that I had travelled sixty-five thousand miles in five and one-half months.

The trip had given me great opportunities to be shown much geology; it strengthened my conviction that to understand the Earth's behavior one should study it as a whole and it gave me a realisation that the Earth's surface details, which enthrall geologists and are vital in economic studies, will only be fully interpreted when fitted into greater, deeper, and as yet undiscovered, major patterns.

One of the first consequences of the trip was to agree with Arthur Holmes, a famous geologist whom I had met in 1948 in London, that Precambrian shields could be subdivided into provinces of different ages. He had just done this for part of Africa and I had now seen several other shields. That same idea also struck Professor J.E. Gill of McGill University and inspired him to produce the first map of the provinces of the Canadian Shield. Almost simultaneously and quite independently, I did the same. We scarcely knew one another, but had used much the same data and had come to the same conclusions. I had similar ideas for other continents.

Holmes had pioneered in dating rocks by chemical analyses, but was anxious to have access so the more precise isotopic results which our students with their mass spectrometer could provide. He agreed that he would send us samples. Thus, it was at Toronto that the first very old rocks to be discovered in the British Isles were dated. Also, we got enough dates from several continents so that R.D. Russell, R.M. Farquhar, and I published in the Handbuch der Physik what I believe to be the first statement that the Precambrian rocks, of all parts of the world, can everywhere be divided into older Archaean rocks which differ greatly from younger Proterozoic rocks.

When students and I compiled maps, largely from air photographs of structures in the Canadian Shield, we could see that many major mining camps, for example, Sudbury, Kirkland Lake, Val d'Or, and Pine Point, are located along great faults, especially at intersections with other faults. Although I did not then realise it, these same maps certainly suggested that the sites of Kidd Creek and Thompson Lake mines would also be favorable. Others may have used the results to help to find those mines.

I was fascinated that such great ore bodies could have been concentrated in so few mining areas. The source of such accumulations of unusual metals could not have been local ground water, but had to be related to deep-seated and still unknown patterns of the Earth's behavior. It was all part of a great puzzle which both attracted and baffled me.

As I saw more and more volcanic islands, the problems of island arcs and particularly why they should be so precisely circular attracted my attention. It seemed that the reason must lie in the physics of failure of rocks, but it was not students and I who found the answer but F.C. Frank, a physicist at the University of Bristol. On the other hand, after being driven nearly to the summit of Mauna Loa volcano on Hawaii and after also reading a report by J. Dana, who in the 1840's had been the first geologist to examine the Hawaiian Islands, I was able to suggest that a volcanic plume or hot spot now rising to the surface at the active volcanoes of the Island of Hawaii had been long-lived and had created the long straight string of those volcanic islands which get progressively younger to the southeast. This implied that the Earth's surface layer had moved across the plume which was fixed in the interior; this added another clue to the solution of the puzzle of the Earth's behavior.

Later, by examining maps prepared at Columbia University of the floor of the Atlantic Ocean and early aeromagnetic maps made off the west coast of the United States and Canada, I interpreted them in terms of great faults and predicted the direction of movement and the total offset of some faults, including that of the San Andreas Fault in California, later confirmed by Lynn R. Sykes at Columbia and R. Bolt in California. In puzzling about world-wide behavior, I used the data provided by many others including Ewing at Columbia, Hess at Princeton, Bullard at Cambridge, and many in California. Most Earth scientists, with a few exceptions, are specialists who either work on specific subjects or in particular areas. As a physicist, I was sure that if strangers from outer space had discovered the Earth they would immediately have wanted to understand its broadest features.

Thanks to an early start, begun in 1948, for fifteen years I was a member of the executive of the largest and most active of all the scientific unions, the International Union of Geodesy and Geophysics. It embraced, among a host of lesser subjects, large scale surveying, earthquakes, geomagnetism, the atmosphere, the oceans, fresh water supplies, and volcanoes. The meetings afforded great opportunities to examine the Earth, and for forty years I travelled, in long-distance flights, an average of about one thousand miles a week, frequently to Europe and many times to other continents. In going around the globe I chose ten different routes. As our children grew up, Isabel was able to accompany me on many of the most interesting trips and the whole family saw something of the world. We had a junk built in Hong Kong and for twenty-five years sailed it on the Great Lakes. It was picturesque and caused a mild sensation, particularly when I travelled in it to Montreal to give a lecture, at the time of Expo 1967. I had bought it because a junk gave the most boat for the money and was perfectly adapted to picnicking in the Georgian Bay. The Chinese had developed junks into safe, seaworthy, and very practical boats with which they had discovered North America in about 600 A.D.

The outstanding event afforded by the International Geophysical Year, 1957-1958, was that the largest meeting was held in Toronto. Since all the natural phenomena studied by geophysicists affect everyone, and because politics has little influence on such phenomena as earthquakes, geophysics presents a perfect subject for opening international exchanges. Because of this I was able to travel to many countries, some largely closed to the outside world and often under fascinating auspices. Thus, at the request of Rear Admiral Lloyd Berkner, USNR, who had been in Antarctica with Admiral Byrd, and Professor V.V. Beloussov representing the Soviet Academy of Sciences, I went as a friendly emissary representing the International Geophysical Year to visit the Chinese Academies of Science in both Beijing and Taipei. At that time, visitors in China were few even from other communist countries. No Americans were allowed and the Canadians had no mission in China. It was fifteen years before President Nixon's famous visit. Three weeks after I attended a party at the British Embassy in Beijing, the Chinese burned it down. The derogatory paper posters, which were fluttering from every square inch of its outside walls at the time of my visit, had added to the blaze. Nevertheless, I was warmly welcomed in China and toured the country widely– a solitary western visitor among countless Chinese. After spending two days in Hong Kong, I flew on to Taipei where members of the Academy in Taiwan welcomed me very cordially. No one mentioned anything about the fact that the two Chinas were shelling one another across the strait which divides them.

Both organizations titled themselves "Academia Sinica", and both claimed to represent the whole of China. I had been sent as a representative of the geophysical community to explain that, qualified as we were to admire and evaluate their scientific work, we were in no position to adjudicate their internal rivalries and, until they settled the squabble, we could admit neither. The problem was that the U.S.S.R. backed the People's Republic of China, and the U.S.A. lined up behind Taiwan. If the committee admitted either academy, either the U.S.S.R. or the U.S.A. would leave, which would be disastrous for plans of world-wide cooperation. Until the Chinese settled the matter, we could not admit either academy. Both academies treated me magnificently and, on that and subsequent trips, I had long conversations with Ko Mo-ro, the distinguished anthropologist and writer who was President of the Academy in Beijing, a member of the Politbureau, and a friend of Chairman Mao, as well as with Dr. Mei, a fine scholar and the Minister of Education in Chiang Kai Chek's cabinet in Taiwan.

I wrote as fair accounts as I could of geophysics in both parts of China and published them in two separate books. At a time of overheated propaganda by everyone, the Chinese evidently appreciated my attempt to tell the truth and made me welcome on later visits.

During a few million miles of travel, I have had many strange adventures and fascinating experiences, but unexpected and surprising as it may seem to those who are accustomed to the wild dangers encountered every half hour by heroes on television screens, it seemed to me that nothing very dangerous ever happened. The two times I remember being frightened were both on big, fast rivers in Canada; on the Abitibi in Ontario I once had a struggle to get ashore, and on the Taltson near Great Slave Lake the fast current nearly swept my partner and me, in our canoe, over a ninety-foot waterfall.

Some authorities, of course, searched my luggage; others tried to blackmail me, but, as I had nothing to hide and I didn't take the bait, nothing happened; it was implied at various times and in different countries that I was suspected of being a capitalist spy or a communist agent, but I was never openly accused nor mugged, and I was only mildly unwell now and then for a single night. My luggage was never permanently lost, except for a roll of published geological maps which I assume was confiscated. As enthralling as all this travel was, it was but delicious icing on a more substantial cake. Travelling when I did and the way I did had given me an opportunity to be shown the world as few people have ever enjoyed; but I was slow to realise that the pieces of the jigsaw that could in time produce the answer to the puzzle of the Earth's behavior lay scattered in the panorama that had been unfolding before me for so many exciting years. It took a long time to change my focus from the details of geological mapping to the search for broad features, and even longer to consider what had caused them. In the 1960's, the development of the theory of plate tectonics seemed to provide an answer, but it is incomplete. Although it provides a tenable theory of what is happening to the Earth, it offers no explanation of why, and so the search goes on.

7. Erindale College: planning a career

In 1967, my research had reached an impasse and I felt like a mountain climber who had safely reached a terrace but was uncertain of the will or the strength to attempt another peak. As if on cue, Claude Bissell, the President of the University of Toronto, asked me to be the Principal of the new Erindale Campus. I said no, but Isabel helped to change my mind. Three hundred acres of rolling land overlooking the Credit River had been set aside for the new college. The only building of any consequence was a fine private house, with a vast run-down garden and avenues of trees, which Isabel thought would provide a splendid setting in which to play the lady of the manor. When we joined Erindale, it had no students, only a planning committee of ten staff; its magnificent campus was almost empty except for a hole in the ground where the Reichmanns of Olympia and York had begun one of their earliest development ventures, the construction of a small building adequate for the first few hundred students.

At this time student unrest was at its height and I had just spoken at several troubled campuses in the United States, at one of which arson had suffocated a student in a dormitory building housing two thousand unsupervised undergraduates. I was ready to believe Dean Peter Robinson, who suggested that it might be a good time to change the design of college buildings. He told me that it was a thousand years since the Benedictine monks had founded their great abbey at Cluny, which, with its surrounding monastery buildings, had provided the general pattern for most monastic and collegiate foundations ever since. It was based on the premise that each institution would be largely residential in nature, with an abbot (or principal) in charge, who was assisted by a chapter of canons (or dons) who lived among a larger body of monks (or students) and controlled their behavior.

In the late 1960's, that was clearly not a suitable plan upon which to build a suburban campus in Ontario. Most students had cars and lived off-campus; they were rebelling against close supervision and they certainly could not be counted upon to go regularly to the same chapel. With the help of two consulting architects, we persuaded the University authorities to scrap their first set of plans and substitute a design which we hoped would better suit the times. It would be a student and a community centre for Mississauga. Instead of dormitories, Erindale would build cheaper, smaller row houses which would require no internal supervision and could be rented to families in summer.

We found that, being only twenty miles from the St. George Campus, we could share professors and the costs of their salaries. They would teach graduate students downtown for half the week and undergraduates with us for the other half. To attract scientists, we built the most modern science labs in Ontario, but instead of duplicating the huge Robarts Library, we only built a small one, persuading those interested in research in the humanities that they could commute to the Robarts Library by our shuttle bus.

There being no facilities for feeding visiting notables apart from the student cafeteria and Isabel's kitchen, the lady of the manor had a busy time. Her successes produced a very happy atmosphere in both the college and the community.

In a marginal way, I kept up with science by continuing to travel and by remaining the editor of an international journal; but after seven delightful years at Erindale I was sixty-five and expected to retire. The Hon. William Davis, the Premier of Ontario, postponed this fate by asking me to become Director General of the Ontario Science Centre, initiating what were to be eleven of the more fascinating years of my life.

Five years earlier, by a stroke of genius, the Ontario government had decided to build the largest science centre in the world. It was in effect a museum of modern science of which a large proportion of the exhibits could be operated by the public, entertaining themselves and learning some basic science in the process. The Centre had opened at the time when people everywhere were realising the importance of universal literacy in science and of teaching it by experiment. The Centre was fully operating when I arrived. That year the Centre attracted one million, five hundred thousand people, of whom three hundred thousand were classes of Ontario school children who had come with their teachers from all parts of the province.

There was no need to change a new and flourishing establishment, but since the only other similar institution in Ontario was the National Science Museum in Ottawa, there was need to provide help to the rest of the province and to assist new science centres to start. The staff suggested that we load vans with specially designed exhibits to travel through the province like a circus, setting up shop in community centres or in shopping malls to provide entertainment as well as education. These soon visited all parts of Ontario, even making trips by air to the most remote Indian bands in the north. From these exhibits grew the idea of dispatching circuses abroad. Large exhibitions were sent to several places in England, Japan, China, Malaysia, and in the United States; they included one which the New York Hall of Science used as its re-opening exhibition after being closed for a few years.

Its success made the Centre a very rewarding and happy place to work in; its size and novelty attracted delegations from all over the world, notably from France, where a huge new centre was under construction in the east end of Paris as a scientific counterpart to the Centre Pompidou for the Arts.

Special summer exhibitions had been a feature of the Centre for some years and the months just before I arrived the staff had mounted "In Praise of Hands", a very successful show on crafts and craftsmen of the world. The public had come in droves, attracted by these demonstrations of elementary science and technology that were conducted by skilled practitioners right before their eyes. The latest ideas in science are impressive, but difficult for most people to understand; they prefer simple science presented in novel ways. Succeeding exhibitions on how plants, animals, and insects practice camouflage or upon the use of robots or on the technologies of Inuit and Indian Canadians were more successful than exhibitions which were either too exotic or too humdrum. In every case, it was most important that the public should be able to participate and that highly skilled people should give demonstrations.

From these early successes sprang the idea of presenting the origin and development of science in ancient China: the source of much western science and technology introduced to Europe by Marco Polo and the Venetian Jesuits. The execution was not easy. It took the visits of three parties of our designers to China and three delegations of Chinese experts to the Centre before an exhibition was organized. Seventeen of the finest Chinese artisans demonstrated such skills as carving wood type, making paper, printing, molding and glazing porcelain, growing silkworms, spinning, weaving and embroidering silk, and the design of earthquake resistant buildings, in as close an approximation to a Chinese market place as the Centre could devise.

In exchange we sent a travelling science circus to China. Their senior educational authorities, some of whom I met, told me that they had the enormous problem of educating no fewer than three hundred million Chinese children and youths, most of whom would never get to university. They realised that science centres would be of great value. They immediately started to design a central one in Peking and invited my successor to its opening. I have just been presented with a book in Chinese with English captions. It shows color photographs and maps of one hundred and seventeen science centres built in all parts of China during the five years since our exchanges. This is an illustration of Canada's potential abilities to influence the world and our custom of keeping our successes hidden. It also confirms China's ability to carry out huge projects quickly, in spite of reports to the contrary.

It is a strange characteristic of science, so vital to our present civilization, that it attracts little attention among the people that it serves. As far as I have seen, the media in Canada have failed to report on these hundred or more versions of the Ontario Science Centre, but had China built one hundred hydroelectric plants or art centres modelled on a Canadian example, we would never have heard the end of it.

8. Retirement

I was very tired when I retired in 1985, and did little for the next two years. I raised my head sufficiently to apply to the Humanities and Social Sciences Research Council of Canada and the Walter L. and Duncan Gordon Foundation for grants, with which I employed students and secretaries to put my own files in order, and to collect and organize all the information available about two northern expeditions named the British Canadian Arctic Expedition staged in the 1930's, and Exercise Musk Ox of 1945-1946, neither of which had ever been properly described.

As I got my wind back, my thoughts returned to geology. In the nineteenth century, geology had been the queen of sciences; Darwin had thought of himself as a geologist. After 1850, geology had begun to lag behind. Physicists, chemists, biologists, and botanists were all eagerly experimenting and theorising, while geologists, absorbed in their meticulous map making and economic preoccupations, continued to plod along well-known paths; their work was limited by an almost total ignorance of the greater part of their domain--the ocean floors and the Earth's vast interior.

In my student days, I had witnessed the revolution in the physical sciences and, as a professor, another example of the same phenomenon in the biological sciences. It was logical, therefore, that I was among those who thought a revolution was due in geology. The main ingredient of any scientific revolution is new information important enough to demand a change in the way of looking at that science. In the physical sciences it had been the discovery of natural radioactivity, relativity, quantum mechanics, and distant galaxies, none of which was explained by classical, Newtonian science; in the biological sciences it was the discovery of DNA and the later elucidation of how the arrangement of its atoms in a double helix gave it the peculiar ability to replicate.

After the second world war, geologists were given the tools better to explore the ocean floors and to probe the Earth's interior. The mapping of the ocean floors goes on apace and, within the past five years, methods have been found to study all parts of the Earth's interior in detail. The theory of plate tectonics was begun in the light of that new knowledge, and geology's own scientific revolution was underway.

The curious thing about a scientific revolution is that any scientist involved in it claims that it has simplified his subject; Dr. Ledermann, a Nobel prize winner and Director of the Fermi Laboratory, insists that physics is becoming steadily simpler. Many would doubt this because of the complexity of the theories and the elaboration of the instruments required to pursue the subject, but what is meant is that the material world we see was first reduced to combinations of some ninety-two elements and that then the atoms of these elements were broken down to combinations of a lesser number of nucleons. These were then smashed into still smaller particles, the number of which become steadily less as the work proceeds.

It is possible that geology too is much less complicated than we geologists have always imagined, and that beneath all the chaotic wealth of detail in a geological map lies an elegant, orderly simplicity.

It has been a brain-cracking search that has preoccupied me these last two years, and my eighty-year-old wits no longer have the stamina and clarity which they once had, but they have come up with an answer of a sort that some reviewers reject and which produces blank, incomprehending stares on the faces of some audiences, but these are reactions I experienced with discouraging regularity in the days when I was first beating the drum for plate tectonics.

It is hard to change anyone's opinion. I have concluded that the key to the behavior of the Earth is that its three outer shells behave in different ways, are governed by different laws of classical physics, and all interact strongly one upon another. The patterns are plain on the relatively young ocean floors, but geologists have tended to concentrate their attention to the parts of the Earth which they can see on land. There the record has become garbled, reworked, and repeatedly distorted over geological time. I think that the key to the Earth's behavior is in the second layer, a strong spherical shell of brittle rock best known from earthquake studies. That shell can only be broken where it is weakened. It breaks in three ways which follow the same laws as those used by engineers in studying structural stress. It ruptures in three ways, one of which is by tension occasioned by the uplift of about forty hot spot volcanoes. These breaks become connected by rifts, as H. Cloos saw in East Africa. They spread apart, as A. Wegener and H.H. Hess proposed, into narrow seas, and then form the mid-ocean ridges, first mapped in their entirety by W.M. Ewing and B.C. Heezen. The other two methods of fracture are shearing along the margins of continents, as in the San Andreas Fault, and by compression under mountains and beneath trenches on ocean floors off mountains, as under the Andes, or island arcs like Japan. Both those kinds of fractures are formed along the margins between continents and oceans. Once all these breaks have started, major convection currents inside the Earth very slowly move the fragments about as is observed.

As my ideas clarified and developed in detail, they have come to be accepted in some quarters, and three papers are to appear in Canada, the United States, and Europe. That does not imply that they are correct, but it is gratifying, fifteen years after retiring, to introduce revolutionary ideas which are palatable to physicists, though still viewed with deep suspicion by orthodox geologists. Others are working on the same lines and more may come round to my way of thinking in time as, over the years they have graciously come to accept transform faults, the Wilson cycle, and other ideas even more outlandish than my present offering; but whatever the outcome, few people have been so fortunate in having enjoyed so happy and rewarding a life as I, and I am grateful to an immense number and variety of people who have speeded me on my headlong way.

J. William Costerton

(July 21, 1934 –)

Killam Laureate, 1990

I was born on July 21, 1934, into a large and closely-knit extended family of farmers and real estate people in and around Vernon, in the Okanagan Valley of British Columbia. My mother, Hilda Costerton, had single-handedly raised herself and four siblings after the death of her mother when she was 13. After my father, Leonard Fisher Costerton, died when I was 8, my mother entered a residential Registered Nursing program at the Royal Inland Hospital in Kamloops. I was admitted to Vernon Preparatory School, as a boarder, and was promptly christened "Cossie II" because my Dad had been one of the redoubtable Mackie's first boarders when the school had been opened in the early 1900's. Boarding school presented many academic challenges and Mr. Lowndes, our wonderful science teacher, had only 5 students in his senior class; so he entered two of us to write our B.C. grade 12 science exams when we were 12 and 13. We passed, we graduated from Prep School, and I went into Vernon to live with my Mom and to attend High School until I got to be old enough (16) to go to University. In four rich low-pressure years, I learned about friendship, carpentry, girls, mining, badminton, sheep-herding, swimming at the quarry, drama, French, singing in Miss Reekie's choir, and I learned about love from Vivian and we planned our life together when we were just 16. It was with Vivian, in the sun-drenched Okanagan Valley and in the sparkling alpine meadows of the Monashee Range, that I began to feel the love that I still have for adventure and for country that is big and bold and beautiful.

From the campus of the University of British Columbia, I could sometimes see the mountains of the Coast Range just for a few minutes before my classes, and I would rest content. I came to UBC with a vague ambition to become a journalist, but Dr. Stock's Introductory Microbiology course was my downfall. He conveyed a breathless enthusiasm for the intellectual patterns that he led us through and an undercurrent of the practical importance of our understanding of bacteria that I still feel to this day. In the late afternoons Dr. Stock would come into our labs, on the pretext of checking our progress, and he would sit on the bench and tell microbiological anecdotes that taught us even more than his excellent lectures. That was it then, Microbiology and Biochemistry. We had a great class, with 5 first-class honours students led by pert red-headed Julia Coppens-Gerwing-Levy, and we aced the Biochemistry courses and triggered special teaching from most of the profs in the department because we showed a very lively spirit. We were immensely happy, in Microbiology at UBC in the late 1950's, and we had no idea how primitive our science was in Western Canada. Amongst all of the

UBC microbiologists only Jack Campbell had real international recognition and we were taught from textbooks - even in our fouth year courses. In 1959, I was taught that bacteria didn't have any nuclear apparatus and probably didn't have DNA! The machinery of science was there, in that we had courses and labs and profs, but discernment was lacking because none of us knew that crystal purity of first-class international science. We knew how to pass an exam, we even knew how to do primitive "one question" research and how to publish a routine paper in a scientific journal, but we didn't know any "hot" scientists at the cutting edges of their fields. Gobind Khorana, later the legendary nobel laureate, was at UBC at that time and he taught us a course in nucleic acid biochemistry, but he was sequestered in the B.C. Research Council and few of us ever really met him.

When I won a NSERC postgraduate scholarship and was forced to leave UBC after a one-year M.Sc. degree, because of their excellent "2 degree" rule, I was certain that I was leaving both God's country and a scientific Mecca. Then I arrived at Dr. Bob (R.G.E.) Murray's very small Department of Microbiology and Immunology at the University of Western Ontario and found myself in the international world of science. Three of the 4 profs (Murray, Robinow, Fitz-James) had just published chapters in the Society for General Microbiology (UK) Symposium on Bacterial Anatomy. Their interests lay in the area of the cell envelope and in the bacterial nuclear apparatus, a structure that had not existed in Vancouver just 6 months previously! Bob Murray edited two major journals and he moved in the forefront of his field. He had learned more Microbiology than I knew at that time, before he was 18, because his father (E.G.D. Murray) and his many microbiological friends talked of current international research at the dinner table. A very young product of Western Canadian Science had met an almost equally young poised product of an eminent scientific father and of McGill and Cambridge, and I was very impressed. Not overawed, not even humbled, just very impressed. And the project I was given was not the routine assignment of that dreary biochemical era - the isolation and characterization of yet another enzyme of E. coli - but it was to explore the whole borderline of living things between the bacteria and the algae! I learned how to use the light and electron microscope to look at bacterial cells and, much more important, I caught Bob Murray's fascination with the big picture. We would simply define forms that were intermediate between bacteria and the blue-green algae, and shift this whole algal class deftly into the prokaryotic kingdom and away from the eukaryotic algae. In Bob Murray's world you might concern yourself with the minutiae of a bacterial structure to understand how the cell works, but you always kept your eye on the big picture. Now, when I see a graduate student submit his miserable thesis on the cloning of yet another gene, the modern equivalent of characterizing yet another enzyme, because his director has too many students and too few ideas, I thank all of the powers for Bob Murray's clear view of the big picture.

After my Ph.D. in Dr. Murray's lab, I spent 4 years in India, where I was an educational missionary for the Anglican Church of Canada, and where I was able again to indulge my passion for adventure and for mountains. I was led to India by a powerful personal experience of our loving God, and I spent 4 very happy years teaching pre-medical science in a small Christian College in the Punjab, with the objective of replacing foreign missionary doctors with Indians in our remarkable chain of hospitals in the Himalayan region of North India. After India I spent two years as a postdoctoral fellow in Botany, in Cambridge, with Enid MacRobbie. Enid is a physicist with a very incisive and disciplined mind, but her very well-funded research interest runs to ion transport in giant algal cells. She is clearly the most brilliant person I have ever known, and she taught me that techniques are entirely secondary to concepts. In her lab I learned that biochemists can apply ion clamps, physicists can assay enzymes, and electron microscopists can study ion transport. We were no respecters of specialties, no one was afraid of any reasonable technique, and we all worked to fit pieces into the big puzzle - how plant cells get ions and how they distribute them among their compartments. Techniques were something that anyone could do, concepts were all important. Because of this training I tend to look someone in the eye and say that it is wonderful that he has made a gold-linked monoclonal antibody against fingernails, or cloned the gene for blue eyes into E. coli, and then ask him what he is going to do with it!

Upon my return to Canada in 1966, Bob Murray found me a job as a professional associate in Bob MacLeod's lab at Macdonald College of McGill University. Solemn chemical studies of bacterial cell walls were underway, but the chemical data were turgid and there always seemed to be a heavy background in the cell wall preparations. So I looked at these cell wall preparations, which were made according to a recipe from a much-quoted paper, and saw in minutes that they were whole bacterial cells - broken but essentially intact with ribosomes and DNA and all kinds of wonderful "background molecules". We went on to do some good work on the cell walls of Bob MacLeod's "stripper" strain of a marine bacterium that shed its cell wall when deprived of divalent cations, but the central lesson of those 4 years on Macdonald College's beautiful campus was that one must never extrapolate from indirect data when direct observations are possible.

As I set up my own small independent lab at Macdonald College, and two years later at the University of Calgary, my training was complete. My learning will go on throughout my life, but my training was complete. My climbing and rugby years had taught me team work skills, my uncles had taught me that dignity was often used to mask abysmal ignorance, my wife Vivian had taught me that joy is to be found in children and in beauty, Enid MacRobbie had taught me that scientific techniques and disciplines were readily interchangeable, and Bob Murray had taught me that direct observation beats extrapolation and that a scientist should move with the leaders at the world level and keep his eye on the big picture. So I was all trained, and happy with our four children and my rugby and climbing, but I didn't know which big picture I was going to address.

The answer came in the form of one Kuo Joan Cheng, who became my post-doc when Bob MacLeod found that he couldn't understand Cheng's spoken English. Cheng's modest objective was simply to understand all of the microbial aspects of the function of the bovine rumen! As a first step, we embedded some rumen contents and examined their bacteria by electron microscopy. The bacterial cell surface structures that we saw in this natural system had never been seen before and we had entered Alladin's cave. Rumen microbiologists were solemn folk and they isolated bacteria from this eminently functional ecosystem and purified and cultured them before they examined them closely. During this culture in artificial media, they lost the ephemeral and beautiful structures that protect them in the rumen and they became just ordinary bacteria - like E. coli! Later we even examined wild cells of really pathogenic E. coli, directly, in the intestines of infected calves and we saw their beautifully complex radial surface structures. So we looked at bacteria in every system we could find, from alpine streams in my beloved Bugaboo Mountains to the less scenic portions of urinary catheters, and we found that all of these natural bacterial populations had elaborate extracellular structures that mediated their adhesion to surfaces and facilitated their formation of matrix-enclosed multicellular biofilms. We submitted an outline to Scientific American and, in February of 1978, the biofilm theory was formally launched. We called the elaborate extracellular structures the "glycocalyx", we described adherent "microcolonies" and "biofilms", and we suggested that their formation was a universal characteristic of bacteria. The phone rang continuously for about two months, and we had offers of collaboration from all kinds of engineers and medical practitioners because what we said agreed with their practical day-to-day observations of corroding pipelines or of infected hip replacements. I took the show on the road and one night, just as I was going to sleep in the Hotel Krasnapolski in downtown Amsterdam, the thought struck me that Pasteur's very old European concept that bacteria must be isolated before they can be studied had served us well but that we were entering a new era with new techniques that would allow us to study mixed populations of adherent bacteria where they actually grow and where they interact with each other. We were entering the Post-Pasteur era of Microbiology.

By the time Gill Geesey's powerful three postdoctoral years in my lab had run their course, we were really coming to understand that bacteria in river systems grow predominantly (99.9%) in biofilms, and that almost all of their important metabolic activities take place in these surface associated populations. River bacteria adhere avidly to surfaces, cover themselves with their slimy glycocalyces, and use this charged matrix to trap nutrients from the flowing water. Snug as could be in their well protected niches, they stay near their nutrient sources and dispatch small numbers of motile "scouts" to colonize new surfaces. We could see these biofilms and we could measure what they did, the picture was and is relatively simple, and Gill and Kevin Marshall and Dave White and Bill Characklis and I could sing and dance in happy unison at dozens of ecological meetings.

The data were predictive and we were able to find biofilms, doing precisely the same things, when we followed the natural water systems into factories and refineries, turbines and pipelines. In aerobic systems the predominant species was Pseudomonas aeruginosa and we had discovered the basic survival strategy of this aquatic organism in both natural and industrial ecosystems.

I was intensely interested in the microbiology of the pneumonia that affects cystic fibrosis patients - a pneumonia caused by Pseudomonas aeruginosa. Subconsciously, I felt that this basically aquatic organism must employ the same survival strategy in the human body that it uses in streams. We used direct microscopic methods to describe extensive slime-enclosed biofilms in the lungs of cystic fibrosis patients and in other chronic Pseudomonas infections, and we concluded that this organism used essentially the same survival strategy in the hostile aquatic environment of the human body. I really couldn't see how this unicellular bacterium could possibly deduce that it was inside of a human body and change its basic strategy of growth and survival. Therefore, I extrapolated. I decided that Pseudomonas biofilms in the human body formed for the same reasons that they formed in natural and industrial ecosystems, and that they were similarly resistant to antibacterial factors. This extrapolation seemed logical and innocent enough to me, but I had crossed over a very well-defended traditional political border between Medical Microbiology and Microbial Ecology and I soon stood naked among powerful enemies. The grand old British men of pathogenic mechanisms were incensed, and Harry Smith actually delivered an organized rebuttal to one of my talks at a Gordon Conference. My respect and affection for Harry made me think very hard about biofilms in chronic infections, but I honestly couldn't see how Pseudomonas could change its basic survival strategy when it entered the body. Closer to home, my medical presentations were received with silent contempt, especially by my medical counterparts in Calgary, and it seemed that I really didn't understand the mysteries of human infections and that knowing the ways of Pseudomonas didn't count for much.

Microbiology is traditionally organized into specialties based on the ways in which bacteria impact upon human affairs and we have industrial, agricultural, veterinary, medical, ecological, and "pure" categories but I have never recognized these distinctions. My ideas concerning Pseudomonas biofilms had been developed in the "pure" and ecological areas, and were widely accepted in the industrial and agricultural areas, but they were clearly anathema in the medical area! However, my anger and frustration were mitigated by glimmers of acceptance when Tony Gristina (an orthopedic surgeon from North Carolina) and John Mills (an endocarditis specialist from San Francisco) agreed to collaborate on biofilm projects and when Allan Ronald, still the doyen of Canadian infectious disease specialists, assigned young Tom Marrie to see if there was anything to this off-the-wall stuff about biofilms! Perhaps because of the prevailing scepticism in the medical field, Tom and I began to use direct morphological methods to prove that bacteria really did form microcolonies and biofilms in a very large number of chronic bacterial infections, especially those that were associated with medical devices. At first, even my closest medical colleagues would only join me to report that biofilms were present in chronic bacterial infections, but soon we were joined by clinicians who accepted their existence and wanted to know what they were doing. Andy Bruce (Urology, Toronto) welcomed my young scientific colleagues into his department and sent Curt Nickel, already a board-certified urologist, to work for a year in my lab. Curt hit the seam between Microbial Ecology and Clinical Urology and ripped off 20 papers in rapid succession, on everything from the antibiotic resistance of biofilms to the role of biofilms in urinary stone formation. Tony Khoury (Urology, Sick Kids, Toronto) followed and presented several fascinating surgical problems for microbial analysis, and now Joe Sung (Gastroenterology, Hong Kong) has joined us to do brilliant work on the Microbial Ecology of the biliary tract and to study for his Ph.D. More clinicians have begun to think in terms of biofilms and now we have powerful allies in the State Serum Institute in Copenhagen (Niels Høiby and Ami Kharazmi) and in the Department of Medicine at the University of Alberta (Garner King and Mrinal Dasgupta). The biofilm concept is eagerly accepted by specific groups of clinical specialists and the Medical Research Council has awarded long-term funding for our basic studies of the physiological bases of the resistance of biofilms to antibiotics. In 1989, Harry Smith, who is a perfect gentleman but an imperfect humorist, came to Calgary to say that he was very glad that I had "stuck to biofilms", and I was glad too.

While Medical Microbiology was providing one penance after another, I had begun to introduce the biofilm concept into a virtual vacuum in Oilfield Microbiology. Oil engineers are well trained in chemical and physical concepts, and they have systematically solved almost all of the chemical and physical problems that their capricious discipline throws at them, but they are essentially innocent of Biology and several important biological problems had gone unsolved. Bacteria plug water injection wells, corrode pipelines, and produce H_2S to "sour" oil recovery operations and all of these bacterial nuisances involve biofilm bacteria. Our successful attempts to solve these simple problems attracted the attention of several oil companies and of the Alberta Oilsands Technology and Research Authority (AOSTRA). This attention took the form of generous funding but, more importantly, it brought Ted Cyr into my office one fateful day in 1986. Ted works for AOSTRA as the coordinator of its University Panel, and he had heard me discourse on a bacterial growth form that is the precise opposite of biofilm bacteria - the very small non-adherent dispersed ultramicrobacteria (umb) that are produced when aquatic bacteria are starved for long periods of time. Ted asked if umb could be used to penetrate deep into reservoir rock that was troublesome because of high permeability and then stimulated by nutrients to return to their full size and exopolymer production and specifically plug these permeable zones. I was sure that umb could be used for this purpose, the patent that protects this technology was issued, and these starved non-adherent cells are now being field tested in a large number of applications where deep penetration and tight plugging can be used as precise tools in this precise trade. AOSTRA was quick to understand the simple biofilm-umb spectrum of bacterial growth forms and their potential for practical use in the oil patch, and this quick-footed agency provided my first research chair with 4 years of welcome relief from routine teaching and administration duties. There are few airs and graces in the oil business and, when a Romanian-trained Libyan Ph.D. in Chemical Engineering narrows his eyes, thinks hard, and agrees that you are probably right about a corrosion problem in his multibillion operation off the coast of Abu Dhabi, you feel millions of miles from the academic world where professors talk and everyone else listens.

When we burst into the open field provided by our biofilm theory, I decided that our group should penetrate deeper into this concept, without tidying up details, until we reached some insuperable barrier or until we reached areas in which the theory did not obtain. We have sustained this rapid penetration for 12 years and most of the important ideas in this incisive sequence have first been mooted in our agricultural work with K.-J. Cheng of the Lethbridge Research Station of Agriculture Canada. Because Cheng works with whole animal systems that can be examined at our leisure, and because Cheng's mind works in inspired surges, we have discovered several basic principles of bacterial growth strategy that have later proved to be universal in all of the ecosystems we study. Together we discovered the specificity and the reversibility of bacterial adhesion to digestible substrata, the stability of native bacterial populations on tissue surfaces, the ubiquity and efficiency of bacterial consortia, and the physiological cooperation of adherent bacterial populations with the tissues that they colonize. Our joint work is a consistent source of concepts that can be clearly defined and proved in his easily manipulated experimental systems and then expanded into the much more difficult areas of Veterinary and Human Medicine. Cheng's mind is a breeding ground of new ideas that pay scant attention to conventional wisdom in Microbial Ecology and, because bacteria operate according to the same strategies in all ecosystems, his concepts should be tested by all Microbial Ecologists in all ecosystems because of his remarkable record of success.

Science is often a solitary business but, paradoxically, the key to good science is people. We think in useful steady predictable patterns when we are alone but when we are together we deflect each other into divergent and convergent channels and we learn things we simply didn't know before. When my lab was small at McGill and in Calgary, I had daily contact with my students, from the ultra thorough Cec Forsberg to the mercurial Joe Lam. As funding for my research expanded, my group became larger and I continued to provide and promote a conceptual umbrella under which some remarkably talented graduate students and postdoctoral fellows, like Cam Wyndham and Gill Geesey, flowered into amazing productivity and commendable independence. Then the lab outgrew its infrastructure and I knew that I urgently needed a lab manager. By this time, I had seen most of the things that can go wrong when responsibility is delegated in a big lab, and I knew that I had to anoint someone whose personality and motivation would make him serve the science and the scientists in what was now a very fast-moving and active group. I chose a new junior technician with only a tenuous grasp of English pronunciation, and this very obvious choice has clearly enabled us to penetrate the biofilm field and never look back. Kan Lam has no sense of rank. He sweeps the floor and washes the coffee cups; he scolds surgeons who treat technicians like scrub nurses; he charms university accountants; he takes tearful discouraged students home for his inimitable steak and Caesar salad; he referees, he cajoles, he dictates and he runs the place. Most of all, he cares. He cares whether we continue to lead the field, he cares about what happens to me and to my ideas, and I care about him. In our team, Kan leads his large band of 12–14 technicians, the postdoctoral fellows hunt in independent packs, and I spend most of my time with my graduate students. These are the people who will challenge the biofilm concept at every turn, and these are the people that will run with it and expand it long after I am doddering around some rain-swept golf course in Victoria with only the vaguest idea of what a biofilm is.

Because I ski and climb and hike, I feel like a successful animal; because I love and have been loved by a wonderful family, I feel like a successful person; because I have changed my scientific field and gathered a wonderful team to explore my beloved biofilms, I feel like a successful scientist. But we don't go into the mountains alone any more than we live alone or do science alone, and the ultimate good fortune is to look briefly over my shoulder at this midpoint of my career and look into a hundred smiling faces and feel their hands on my shoulder and my arm as I come to the podium and then get quickly back to my work.

MARIA DARIA HAUST

(August 18, 1921-)

KILLAM LAUREATE, 1990

M. Daria Haust was born and received her primary and secondary education in Poland, graduating just prior to World War II. From the earliest years in secondary school, she enjoyed languages (even Latin!) and literature, but her love affair was with poetry and music. The assessments at school, however, were highest in a different area: mathematics, physics, and chemistry. Even today, Daria Haust does not understand that dichotomy, as she was never fond of these subjects. Moreover, as long as she can remember she has been unable to manage the simplest additions, multiplications, and divisions.

In Daria Haust's mind, the period of her youth is one of the most beautiful episodes of her life: enriching, enchanting, discovering, "blue-skied and white-snowed", "pink-dreamed", and full of romanticism which permeated every phase of life. Even the songs of the boy-scouts were ballads full of mystique ("Na polanie dogasa ognisko —, gwiazdy zgasly i ranek jest blisko, a ty śnisz tęczowe sny." — "In the field, the camp-fire is slowly 'going down' —, the stars are extinguished (now) and the dawn draws closer, but you (still) dream rainbow-like dreams.")

The proud nationalism of free Poland, upon reconstitution after World War I, expressed itself in old, but modified, hymns sung in churches, and the national romanticism was reflected in new songs ("I póki kropla jest w Bałtyku, polskim morzem będziesz ty, bo o twe wody szmaragdowe plynęla krew i nasze łzy" — "And as long as a drop (of water) shall stay in the Baltic, you will remain a Polish sea, because our blood and our tears kept flowing for (on behalf of) your deep-blue waters"). And always, the songs, poems, and the literature were bound to the land, its mountains, streams, and fields. Thus, the love for and the bond with nature was a prominent component of one's make-up.

This period of beauty and broad education was also full of innocence which sheltered one from even an inkling that the world did not consist solely of poetry, dreams, and Chopin's music. It also lacked any preparation for the practicalities of life, a lack that was sorely realized in the years to come.

At the end of the war Daria Haust found herself in Germany, near Heidelberg. Learning of the reopening of the University, she decided to sit the admission examinations for Medicine because she wished "to learn how to tend to the sick and the hungry people" in preparation for return to her homeland. The University of Heidelberg was the first in post-war Germany to open its doors, and was flooded with applications. Preference was given to the German men returning from the war, and, as only a few slots in Medicine were reserved for non-Germans, Daria Haust considered herself fortunate to have been admitted after passing the entry examination for foreigners. To support herself, she worked night-shifts and weekends in the nearby American hospital. As a student, Daria Haust enjoyed particularly paediatrics and obstetrics, and her hope was to concentrate in her future general practice on these two areas. As a student, she also attended lectures in philosophy, and developed appreciation for baroque music, particularly that of Bach.

Daria Haust passed the final university examinations in June, and the orals (with magna cum laude) of her M.D. thesis ("Primary Pulmonary Sclerosis in an Infant") in August, 1951. The thesis was based on investigations into the nature of the obliterative process in small pulmonary arteries in an infant who died while she was summer intern in a local hospital. Little did she know that this work, carried out under the supervision of Professors Velten and Randerath in Pathology, would "tilt the scale" of her professional plans later.

Towards the end of her studies, it became apparent that it was unreasonable to return home "to practise medicine amongst the ill and the hungry", in the face of the annexation of her homeland by another country. Since practising medicine in Germany as a foreigner was equally impossible, the remaining option was to emigrate. At that time most of the graduating foreign students in Germany emigrated to the U.S.A., and Daria Haust found herself in the process of completing the necessary steps for this purpose when the course of her plans was changed. Upon learning of the above plans, an educated senior American officer, a patient in the American hospital, made the fateful statement: "In my view, for someone like yourself, it would be easier to settle in Canada than in my country. On my fishing trips to Canada, I have been impressed by the gentle nature of her people, the courteous customs, and a definite imprint of British culture. I doubt that you would be happy amidst the hard, quickly moving, and competitive life of the U.S.A.". Since the statement was made by a respected leading U.S.A. personality, the advice was taken seriously. Thus began a period of feverish reading about the land called Canada, as, except for geography, one knew little of that country. What emerged gradually from studying her history, social structure, and customs, appeared most attractive: Canada must be a land of broad culture, because it was bicultural; it was a beautiful pristine land with a blue sky and white snow (just like one's own homeland!); and there was a (real!) Eskimo and Indian population, amongst whom one could live and serve with as much devotion as one intended for the people back home. Enthusiasm grew by the day and, ultimately, an application for emigration was submitted to the Canadian Consulate in Germany. During the waiting period for the "Canadian decision", another unexpected event took place: A colleague, two years ahead in medical school, "declared" himself to Daria Haust. Since she was determined to proceed with emigration, he decided to join her. They were married, and spent four months as interns at the University's Surgical Hospital in Marburg prior to leaving by ship in January, 1952, for Canada.

The naiveté regarding their future in the new land was — in retrospect —

pathetic. At that time, all the advice and assistance by the Canadian Consulate consisted of: "Canada needs physicians", and a letter of introduction to the Employment Office in Montreal (the destination selected because the couple's command of French was better than that of English). Each emigrant was permitted to leave Germany with a tiny sum of U.S. dollars; in addition, the couple brought to Canada a wooden box with second-hand (pre-war) medical books and two changes of clothing — none suited for the severe winter in Montreal where the 3-day journey by train brought them from Halifax. They did not know a soul, and no one met them. At the train station they were advised to call a guest-house that at times let rooms to immigrants. The Employment Office was unable to provide them with a job (let alone a post in a hospital), or to explain how to begin to practise medicine. In desperation, the decision was made to open the telephone book and call up the first physician whose name sounded either Polish or German, and ask for advice. The choice fell on Dr. Joachim Brabander. And one could not have been luckier! Dr. Brabander, a second-generation German-Canadian, came to collect the hungry, cold, and downhearted new arrivals; he and his charming wife became saving angels. It was only now that the immigrants learned of the endless steps necessary for foreign graduates to obtain a licence to practise medicine in Canada; the rotating internship in a teaching hospital; registration with a Provincial College of Physicians and Surgeons; the various examinations set by the Colleges for foreigners, and the examination by the Medical Council of Canada. It all sounded like an insurmountable mountain and years of attempts at climbing it. Had the newly-arrived possessed means for a return passage to Germany, this could have been the solution, especially, because Daria Haust's husband, Heinz, was able to practise medicine in his native Germany. However, it would have been beyond anyone's dignity to return defeated. Moreover, since the Canadian Government lent $240 to each for the passage to Canada, this sum had to be returned first!

Dr. Brabander's action was swift; he arranged for immediate employment (Daria Haust became a companion to one of his very well-to-do patients, and her husband became a technician in the Blood Bank of Royal Victoria Hospital) for the transitional period, *i.e.*, until the time to begin the rotating internship. He advised continually how to proceed with applications for the internship and to the Colleges of Physicians and Surgeons, explained the customs of the new country and the taboos of professional life. The latter is vividly exemplified by the following. Already accustomed to the cosmopolitan life of Montreal and grateful for a few newlymade friends, one hoped to be accepted for an internship at one of the hospitals in Montreal. Instead, the first invitation for an interview arrived from the Kingston General Hospital where an agreement for a rotating internship was signed. The interviewer was a young Lecturer in Medicine, Dr. Wilson, who recently retired from his position as Dean of Medicine at Queen's; he also became "a historic person" and a symbol of Canadian values. His integrity and quiet dignity will always be admired.

Upon returning to Montreal from that fateful interview in March, 1952, the newcomers received offers for internships from three teaching hospitals in that city, and the temptation to accept one was considerable. However, Dr. Brabander, who was consulted, advised: "In Canada, an honourable man does not break an agreement. Reputation and respect are lost if a person does not keep his word. In the medical profession this may be particularly damaging in Canada". This was important advice on the path of a new life in a new land, and the newcomers have been eternally grateful to Dr. Brabander and his wife for their guidance. To this day both couples remain in touch, and throughout the years when Daria Haust was invited to lecture at McGill University, Dr. Brabander, a faculty member in Medicine then, was often in the audience.

Finding oneself late in June, 1952, as a rotating intern in Kingston General Hospital (KGH) denoted the beginning of an important chapter in Daria Haust's life. It was painfully apparent that, unlike in Germany, her chosen field, medicine, was a man's world: there was not a single female physician on the Faculty of Medicine at Queen's University, on staff at either of the two (teaching) hospitals, or practising in town. The only other female intern was Hungarian, but she had the advantage of being a Queen's graduate. Similarly, the classes of medical students consisted solely of men, and only occasionally were up to two women included. Whereas there were no female professors on faculties of medicine in Germany either, approximately 40% of the medical students were women as were some 25%-30% of practising physicians. The scene was simply unreal, as was the "treatment" originally received from male interns, the attending staff and nurses (not that anyone was ever impolite).

There were also, at the time quite painful (today: hilarious), "incidents". Unaware of customs and social comportment in Canada, Daria Haust could not understand why she was not greeted by her fellow interns or senior staff. It was not until in answer to her specific enquiry Mrs. Ford Connell, the wife of the Chief of Medicine, laughingly explained: "In our country, it is always the lady who greets the gentleman first, not vice versa". And she added: "Now I understand why all staff think that you are a very conceited young lady, because you never greet them!". Acceptance and spontaneous gestures of friendliness began appearing in Kingston, but slowly. Some came from the least expected quarters, *e.g.*, Dr. Malcolm Brown, who remained a patron of the newcomers even when, upon leaving Queen's, he assumed the important post at the Medical Research Council. Once, while on night duties, Daria Haust found herself alone in the interns' common room with the senior in medicine, the quiet and respected Dr. Don Baxter. Taking heart, she posed the question to him: "Why is it, that after a considerable period spent here, everyone still treats us as if we were strangers?" The answer was quite revealing: "We here in Canada have been accustomed to immigrants from continental Europe who were uneducated labourers and farmers. Whereas they worked hard and were respected (for it), there was little interaction between them and the Canadians whose roots were in the U.K. These immigrants kept up a kind of voluntary segregation by maintaining their own schools and language, and not bothering to learn English. Only during and after World War II have we been faced with immigrants of a different type, like yourselves: educated and in commmand of our language. Please be patient and give us some time to adjust ourselves to the novel type of immigrants".

Dr. Baxter made an important point: one had to be less impatient, and only by "doing one's own thing" unobtrusively could one hope for a full acceptance, to document that, one had to offer the new country one's own cultural contributions and values.

Upon completion of the rotating internship (interrupted by the birth of a baby boy and a 3-month period in Pathology), Daria (as she now became known to her colleagues and friends) stayed home and tended to her small family. She utilized that time for preparation for the required examinations that would in the end provide her with the licence to practise medicine (still: in the North, amongst the Eskimos and Indians). The arrival of a second baby boy (in 1955) brought yet another change of direction when Daria's husband decided to pursue an academic career by enrolling in a Ph.D program at the Queen's Graduate School.

The above change meant that the basic purposes of Daria's entering medicine were, at least temporarily, invalidated. But she now had two little healthy boys of her own who needed her! This period of staying home and taking care of her family was a time of happiness and fulfilment. Whereas she continued to study for her qualifying examinations (and passing them successively) preparing herself to practise "when the children grow-up", for the time being she had no intention of pursuing medicine.

These happy days of family life were short-lived and what followed was not of Daria's doing: she began to be pressured from two sides, to return to work and assist with research in atherosclerosis in the Department of Pathology. The research was conducted by Dr. Henry Movat under the sponsorship of Dr. Robert H. More, the Chairman of the Department. Both the shortage of young people interested in research and Dr. More's desire to develop the Department into an Institute of national and international reputation were factors in attempts at recruitment of anyone with even the least of qualifications. And Daria happened to have written an M.D. thesis on arterial sclerosis which was supervised by two professors of Pathology at Heidelberg University! Daria's husband was also a strong proponent of the proposal. She resisted the offer for 6 months, but ultimately gave in, and agreed to work part-time (3 hours/day) only.

Soon, stimulated largely by the brilliance of and excitement created by Dr. Henry Movat, with whom she shared the tiny laboratory, Daria began returning to work after serving supper and settling the children for the night (with her studying husband baby-sitting). She became fascinated with the cells of the arterial wall under the scrutiny of the microscope, imagining them in a living and functional state, and determined to demonstrate by specially adapted methods that the imagined function of these cells was real. To this day Daria considers Dr. Movat as a major force in awaking her curiosity, scientific approaches, and the desire to master (or introduce new) techniques suited to test a theory.

At the end of that academic year, Dr. Movat was leaving Kingston, and Daria's plans were to return home to her family. But Dr. More, whom Daria saw seldom while working part-time, invited her for a chat just prior to Dr. Movat's departure. He offered her a full-time appointment in research and enrolment for an M.Sc. (Med.) degree in the Graduate School under his supervision. Already "hooked" on enquiries into the nature of the cells in atherosclerotic lesions, as well as on the role of various blood proteins in the inception and progression of these lesions, she had, however, great misgivings about leaving her little boys for a whole day ; citing that reason, she declined. Dr. More, proposed a compromise: Daria would work in the morning, go home for the afternoon, and return to continue in the evenings. As she was returning already for several months to work at night, the proposed arrangement was acceptable. At first, she missed Dr. Movat's stimulating presence, but with passing of time her own work began to "consume" her. It was during that year that she developed and published a new method for tissue processing using tetrahydrofuran, which was adopted in many laboratories in Canada and the U.S.A.

Convinced that research was Daria's future, Dr. More requested that she apply for a "Life Insurance Fellowship in Research"; one of these (ten or twelve) USA Fellowships was awarded annually to a Canadian. The competition (writing an essay and an interview) was scheduled in Montreal for all Canadian applicants, and candidates were advised that their sponsors would be notified by telephone of the results.

Upon returning to Kingston, Daria was told by Dr. More: "I just received a call informing me that you are the Canadian winner of the Fellowship. As much as I am proud of you, I declined (on your behalf) to accept it and advised them to award it to the candidate who 'scored' as Number 2". Daria was speechless! She had spent four days (travelling by bus and sitting for the competition) away from her family, upon the explicit request of her superivsor that she should apply for the Fellowship, and upon receiving it, he unilaterally cancelled it. Why? Later, Dr. More asked: "Are you not curious why I changed my mind and did not wish you to accept that award?" Thus, she learned that Dr. More's action, as always, was dictated by his desire to direct his "disciples" to realize their full potentials, and not "create" in excitement (what he called) "second-class pathologists", *i.e.*, pathologists, who may be gifted, brilliant investigators, but who never completed the necessary "training" to qualify for sitting the Specialty Examination of the Royal College of Physicians and Surgeons of Canada (RCPS). He knew of such instances too well, and after Daria left for that competition, he decided that should she win that award, it would indicate that she would be capable of continuing with formal residency training and passing the Specialty Examination. While explaining all this to her, he also stated that until then there was no complete program of residency training in General (*i.e.*, anatomical and clinical) Pathology at KGH, but he hoped to "create" that, "as you would require it". Dr. More continued: "If you are gifted in the area of scientific research, you will never cease to pursue it, even during the years of your formal training". And he added wistfully: "Should I succeed in devising a complete residency training program for you, you will be the first female resident in that position, and I trust that you will remember the sensitivity of male residents under your command; no doubt they will at first resent being under the supervision of a female colleague, but it is you who must put them at ease and earn their respect". That advice offered by her admired teacher and wise mentor was never forgotten, and guided Daria's conduct towards all male colleagues throughout her career.

Appreciating how important the working relation is between the residents at various levels, Daria always regretted that in her own residency in Pathology she never had a senior "above" herself to lead and advise her. The exception was the 3-month period spent in Pathology as an "exchange intern" at KGH in 1953 (*i.e.*, just prior to the birth of her first child). At the time, Dr. Howard Steele, the resident in the Department, introduced her to routine work on autopsy service and supervised her completion of "cases". He was another "quiet Canadian" who will be remembered for his gentle conduct and tactful teaching of a total "ignorama", and for his modesty despite his enormous knowledge. He returned to the Department in the late fifties, and to this day (with his wife, Mary) remains an admired friend.

The now charted professional course proceeded as planned; following the year of full-time research and course-work (1956–1957), Daria spent the next two years (1957–1959) completing the residency program at KGH. This time will be remembered for its enchanting camaraderie with the (all male) residents under her supervision and those in clinical programs, and for evolving life-long friendships. She will always appreciate the courage of Dr. More in appointing her as the very first female resident for a complete "training" program, not only in pathology, but in any discipline at KGH.

Evaluation of research and writing of the ("major") thesis ("The Role of Blood Proteins in the Pathogenesis of Arteriosclerosis") continued during the residency years, and Daria obtained her M.Sc. (Med) degree at the 1959 Spring Convocation. The "minor" thesis was accepted at the time of her course work (1956-1957). This thesis (an extended essay, not based on original research), was selected in Biochemistry, as she believed that her knowledge in that important field was deficient. The topic was: "The Urea Cycle and Its Implications in Health and Disease". This educational experience in Biochemistry later facilitated her comprehension of diseases of inborn errors of metabolism.

It was a time for subspecialization and, upon completion of her residency at

KGH (1959), Daria spent 14 months as a postdoctoral fellow at Children's Hospital in Cincinnati. Here, under the tutelage of Dr. Benjamin H. Landing, she was to study paediatric pathology. Since the desire of her student-days to practise paediatrics and obstetrics was not realized, becoming a paediatric pathologist seemed to be a reasonable substitute. While in Cincinnati, Daria travelled to Toronto to sit (and pass) the RCPS qualifying examination in Anatomical and Clinical Pathology in the fall of 1959. According to Dr. More's thinking, she was now not in danger of being a "second-class" pathologist.

In Cincinnati, Daria missed her family, visiting often on weekends by endless night train travel. But there was compensation in the exploration of a new field and the added excitement of learning first-hand of recent discoveries, *e.g.*, the chromosomal alterations in the Turner, Klinefelter, Down and "superfemale" syndromes, and the new vistas in inborn errors of metabolism. It was also the time of Daria's return to clinical medicine: examining children, making ward rounds, and attending grand rounds. She learned that one could not practise paediatric pathology meaningfully without a thorough (albeit at times only theoretical!) knowledge of clinical paediatrics, and made a resolution: she would bring that clinically oriented "attitude" of a (paediatric) pathologist to Pathology in Canada!

Stimulated by the genius of her new supervisor (a "quiet American"), Daria continually broadened her interests. In addition to acquiring service-skills, she completed (and published with Dr. Landing) 4 "projects" on diverse subjects: "Osteosclerosis of renal disease in children (27 patients)"; "A new method for localization of acid mucopolysaccharides in Hurler's disease"; "Reaction of phosphoryl chloride with (17) tissue components"; and "Histochemical studies of cardiac 'colloid' (36 hearts)". She extended her USA stay by a six week period for an intense study of placentae with Dr. John Craig at Magee Hospital at Pittsburgh. There, she also participated in educational activities of the Children's Hospital, was introduced to techniques of immuno-histochemistry by Dr. Joe Vaskez, and practised these using the facilities at the University's department of Pathology.

By the end of this intense period of learning, Daria was ready (and glad!) to be returning to Kingston where, in 1960, she was appointed Assistant Professor in Pathology (Queen's) and Paediatric Pathologist at the KGH. She was the only female physician on the Medical Faculty at Queen's and the KGH staff. The service and resident training programs in paediatric pathology and those for postdoctoral fellows and graduate students were soon established. True to the spirit of her Cincinnati days, she forged a close association with colleagues in relevant clinical departments, *i.e.*, Paediatrics, Obstetrics, Psychiatry, and Opthalmology. Contact was initiated and maintained with the Medical Director at the facility for mentally impaired patients at Smith Falls, Dr. H. Frank. Collaborative investigations began with him and Dr. A. Bryans, Professor of Paediatrics at Queen's. Moreover, contacts with colleagues in Cardiology were reestablished because of Daria's sustained interest in atherosclerosis. In this field she continued to collaborate with Dr. More, now a colleague (rather than a supervisor) and later a personal friend. This friendship (also with his wife Dorothy) has stood the test of time (*i.e.*, since parting 23 years ago). Collaborative research soon began extending beyond Queen's University, and involved the Guelph group (vide infra) and later Dr. S. Renaud (Montreal Heart Institute).

With Dr. More, Daria was invited in 1962 to join the group carrying out experimental atherosclerosis in pigs at the Ontario Veterinary College at Guelph, where Dr. Harry Rowsell was the veterinarian pathologist and Chairman of the department. Thus, whenever the pigs were to be autopsied, Daria travelled on the evening train of the preceding day from Kingston to Toronto; holding on with great care to the precious fixative (osmic acid) for electron microscopy (it had to be transported in cool temperature, — ice), she was embarrassed when the passengers noticed the fumes of the acid or the "dripping" of the ice. In Toronto, she was met by Dr. Fraser Mustard, in whose family home she spent the night and with whom she travelled next morning by car to Guelph. Following a day of hard work that was seldom completed before midnight, Daria, returning with Dr. Mustard to Toronto, spent another night at the Mustards' home, and departed on the morning train for Kingston. Here, the processing of tissues and data collected in Guelph were completed and evaluated. The car trips with Dr. Mustard afforded time for stimulating discussions and formulating of theories of atherogenesis worth testing. Those of mutual interest to the Guelph and Kingston groups, *i.e.*, thrombogenesis, were considered for submission of a joint grant application in support of collaborative research. Regular "exchange-visits" between the Kingston and Guelph groups (Drs. Rowsell and Mustard) followed. These were exciting "sessions" full of new vistas and visions (as well as speculation!). Dr. Rowsell (later the founder of the Canadian Council on Animal Care and at present its Executive Director) has remained a dear friend. On occasion, Daria is reminded teasingly how, unaware of conditions at autopsies of large pigs, she arrived on her first visit to Guelph in high-heeled, open-toed shoes and "proper" attire — both totally unsuited for the high water level on cement floors and the splashing of blood!

The time as a member of Queen's University and KGH (1960–1967) will be remembered as a productive, exciting and fulfilling period of growth, both professional and personal. This was a time of cultivating old, and making new friends; of family picnics with friends and their children; of festive events in the closely knit Department of Pathology, in clinical Departments, and those of other faculties; of being part of the life around Queen's and KGH —, yes, of all that one called one's home. It was also gratifying to partake in a new phase of the Department which, still under Dr. More's leadership, gradually acquired status equivalent to that of an Institute, with a national and international reputation. In his efforts, Dr. More was supported by his former "disciples" who, upon specialization, began returning home, and by a well known and innovative investigator, Dr. Sergio Bencosme, who was generous in sharing his ideas. This "giving and taking" was a hallmark of that happy time.

The work at Queen's consisted of seemingly separate and yet interdependent segments. The growing service provided the basis for programs in paediatric pathology for residents in pathology, clinical paediatrics, and in obstetrics (neonatal pathology). (Two paediatric residents, after spending one year in paediatric pathology, "switched" their career to the latter.)

The second component of Daria's work related to supervising the research of postdoctoral fellows and graduate students, and to small projects assigned to every resident spending a year in the service (paediatric pathology). This included supervision of writing of theses, manuscripts, and abstracts for presentation. Basically, Daria confined herself largely to delivering invited addresses, whereas the papers based on work at Queen's, as of 1961, were presented at national and international meetings by "her" young people.

Daria "reserved" certain domains of research for herself, as she was fearful of "losing touch with the bench". Newly available techniques were introduced into her laboratories (immunofluorescent antibodies; ferritin conjugated antibodies; high power resolution electron microscopy; histochemistry; histoenzymology) shortly upon her return, and these were used in several investigations.

Daria also became intensely involved in teaching medical students in pathology and paediatrics. She was "thrown" into teaching very early in her career (assisting in Anatomy, 1955–1956). Since 1956, Dr. More, short-staffed in those days, would often "wander" into Daria's lab late in the morning: he had to leave on the noon train (for Toronto or Montreal) and she would have to lecture to medical students at 1:00 p.m. At first, her reaction was pure horror! Brought up in the European tradition, that lecturing to students is an honour reserved only for the Professor, she reacted at two levels: first was the fear of her own inability to fulfil such an assignment (on such short a notice and on a little-known subject; she had only one or two hours to study it!). Secondly, she was indignant (on behalf of the students) that this important responsibility was delegated to an "underling". Gradually, she lost her apprehension about standing in front of a class lecturing, and soon established a special bond with the young students, who elected her as the best lecturer (in Pathology, or of all teachers) almost every year. In later years she was in charge of teaching content and assignments, and taught herself much General and Special Pathology. She introduced a series of lectures in Paediatric Pathology that were "a first" not only at Queen's, but in Canada. In 1965 Daria was promoted to Associate Professor in Pathology. That same year she was one of three Canadians invited to become a Charter Member of the Pediatric Pathology Club (later: Society for Pediatric Pathology) whose total number of members in North America amounted to 37.

Leaving Queen's and Kingston in 1967 for London (where Daria's husband accepted a new post) was one of the most painful experiences of her life, and a true uprooting of the entire family. Daria speaks sparingly on that subject. Just prior to leaving Queen's, the group working in her laboratories consisted of eight technicians, two graduate students, two postdoctoral fellows, and two (career) paediatric pathology residents.

Initially, Daria resolved not to work in London, since she wished to settle down with her family and remain at home. However, Dr. A.C. Wallace, then Chairman of the Department of Pathology at UWO, persisted in his offer of an appointment, and once again, against her own wish, she consented. Academic pathology in London in 1967 was at its beginning and the tiny nucleus of the UWO Department was housed in the Department of Anatomy. With almost no facilities available for research, Daria found herself beginning from scratch. Fortunately, four of her Queen's staff transferred with her to London (two technicians, a graduate student, and a former graduate student). With their help, "the building" proceeded swiftly. In addition to establishing an electron microscopy suite and her own laboratories at the University, Daria with "her people" (as she fondly referred to her co-workers throughout the years) "created" a small department of Pathology of her own at the Children's Psychiatric Research Institute (CPRI), where she accepted a position as Consultant. The laboratories, offices, and an autopsy suite (with a cold-room!) were self-designed and constructed by the staff at CPRI. Here, she planned to create a centralized Department of Pathology for children with mental and physical disabilities servicing the entire region of Southwestern Ontario (SWO). For this purpose, she travelled to those facilities where the patients were housed and met the administration, coroners, OPP members (to obtain permission for the transportation of deceased patients to CPRI), and representatives of the Ministry. Thus, a network of communications between the provincial facilities and the CPRI was established. Before the construction of the Department of Pathology at CPRI was completed, Daria and/or her assistant (Dr. Choi) travelled to the facilities, at times performing autopsies on the table of a friendly undertaker. These were days of pioneering and improvisation but progress was gradually evident, as the number of autopsies at CPRI rose to 20 in the fourth year of operation. This service became the nucleus for very valuable material for teaching residents; for research in mental retardation and inborn errors of metabolism of postgraduate fellows and graduate students, and of interdisciplinary work with colleagues in Biochemistry, Paediatrics and Neurology. In 1968, Daria was promoted to Professor of Pathology and appointed to the Department of Paediatrics (UWO). She was also a Consultant in the Department of Pathology at Victoria Hospital, which provided pathology services to the War Memorial Children's Hospital. Utilizing the tissues derived from the paediatric age group, she soon taught the residents (in pathology and paediatrics) paediatric pathology at the microscope level and by special seminars; two residents of this time chose paediatric pathology as their career. Daria instituted monthly clinical-pathological conferences at CPRI, and the first weekly seminars in the Department of Pathology (UWO).

In the year 1970–1971, while the Chairman was on sabbatical, Daria acted (with Dr. James Paterson) on his behalf; she succeeded in recruiting the first neuropathologist for the Department and obtained support for part-time services of another neuropathologist at St. Joseph's Hospital.

From the beginning, Daria was assigned much teaching of medical students at UWO. She continued to enjoy this aspect of her career, and later was in charge of teaching for many years; she also served on the undergraduate medical education committee at UWO, and was active in the education of graduate students. In the academic year just preceding her departure for Oxford (vide infra), four graduate students were completing their theses under her supervision: two in M.Sc. and two in Ph.D. programs.

Her sabbatical year (1972–1973) in Oxford was spent in the Sir William Dunn School of Pathology, where Daria worked in the old but then rather inactive laboratory of the late Lord Florey. Entirely on her own (and with financial assistance for materials and supplies provided by the Medical Research Council of Canada), she developed tissue culture techniques for arterial endothelium and smooth muscle cells (in addition to carrying on regression studies in rabbits made previously hypercholesterolemic). Upon returning to London, she established this technique in her laboratories. While away in Oxford, Daria was appointed Professor of Paediatrics at UWO (1972), and upon her return, Staff Pathologist at the newly opened University Hospital (1973), Consultant in Pathology at the Hospital for Sick Children in Toronto (1975), and Professor of Obstetrics and Gynaecology at UWO (1977). The latter appointment reflected her increasing involvement in collaborative investigative and educational endeavours with that department.

International contacts began early, with Daria's first presentation at an international meeting in 1956. Since then, and while still in training, she presented her original work at annual meetings of several Societies in the U.S.A. At these, Daria met young people from all over North America, and many of those early contacts are still being maintained. Since joining the faculty at Queen's, she has been invited on many occasions to lecture to medical students and faculty at universities in Canada and abroad, and to participate in numerous scientific programs at meetings of Societies, Symposia, Congresses and Workshops in Canada, and all over the world (except Africa). Similarly, she was often invited to organize and/or chair International Conferences, Seminars, Symposia, Courses, Contributed Paper Sessions, and Workshops in Pathology, Paediatrics and Obstetrics, at national and international events.

Daria has served on the Editorial Boards of nine international Journals (the first and only female member on the Board of Virchows Archiv, established in the last century), as a guest Editor, and as an ad hoc reviewer for other scientific journals (*e.g.*, Science; Laboratory Investigation). She still serves on the Editorial Boards of three journals.

Daria was the recipient of (full or partial) Senior Research Associateship of the Ontario Heart Foundation (OHF) (now: Heart and Stroke Foundation of Ontario; HSFO) (1960–1987) and a Visiting Scientist Award from the Medical Research Council of Canada (MRC) (1972–1973). She has served as a member of the Grant Review Committees of the MRC; OHF-HSFO; Canadian Heart Foundation (CHF); NIH (Heart and Lung Institute; SCOR-Atherosclerosis and Special Study Section), Bethesda, Maryland, and as an ad hoc Reviewer for other granting agencies (*e.g.*, Ontario Mental Health Foundation (OMHF); Ontario Ministry of Health (OMH)). She herself has held major term or annual grants from the MRC, OHF-HSFO, OMH, and from OMHF. Smaller grants have been received from other Foundations and as private donations.

Daria was elected to the Council of: the American Heart Association (Council on Arteriosclerosis); International Academy of Pathology; Society for Pediatric Pathology; International Paediatric Pathology Association (Chairman); and the Canadian Society for Clinical Investigation. She has served on key Committees (Education; Long Term Planning; Constitution; Nominating) of these and other (Canadian Atherosclerosis Society) Societies; she was elected Secretary-General of the International Paediatric Pathology Association (for two terms), Secretary-Treasurer or Treasurer of the International Atherosclerosis Society (for 3 terms), appointed Chairman of Expert Panel #28 of the International Pediatric Association (for two terms), and elected President of the Society for Pediatric Pathology, International Paediatric Pathology Association, and the Canadian Atherosclerosis Society (CAS). Whereas she completed her 3-year term on the Council of the Canadian Society for Clinical Investigation (CSCI) in September of 1989, she continues serving as the Liaison Officer of the CAS with the CSCI and the RCPS, and on the Scientific Program Committee of the RCPS; she also continues serving on the Education Committee of the CAS, and on the Executive Committee of the IAS. Daria is a Fellow of the RCPS(C), a member of the College of Physicians and Surgeons of Ontario, and a member of many provincial, national, and international professional Societies (medicine; pathology; paediatrics), in addition to those above.

Honorary (or Foreign-Corresponding) Memberships were awarded to Daria by the European Atherosclerosis Group (Society) (1971); the British Atherosclerosis Discussion Group (1972); the Hungarian Atherosclerosis Society (1977); the Hungarian Society of Pathologists (1980); the Chilean Society of Pathologists (1988); and the Cuban Society of Pathology (1989). On November 18, 1989, Daria was elected to membership in the prestigious Heidelberg Akademie der Wissenschaften (Heidelberg Academy of Sciences). In his letter the President of the Academy wrote (freely translated): "You were elected not only because of your many innovative and original contributions to science , but also in recognition of your outstanding efforts on behalf of the International Community (of Scientists), and particulary those of 'integrating' the knowledge in medicine of Europe with that in North America". Other honors and awards received are : Alexander von Humboldt Distinguished Service Award, Berlin, 1982; "Service Award" from the

American Heart Association, Rockville, MD, 1986; "Service Award to the First President" from the CAS, Toronto, 1986; "Distinguished Colleague Award" from the Society for Pediatric Pathology, Chicago, 1987; a "Gold Medal" ("...to express our gratitude for the pioneering, enthusiastic, and dedicated work for the International Atherosclerosis Society, and to pay honour to outstanding scientific research in the area of atherosclerosis"), Rome, 1989; the Isaac Walton Killam Memorial Prize in Health Sciences from the Canada Council, Toronto, 1990; and the prestigious "William Boyd Lectureship" for 1990 from the Canadian Association of Pathologists (CAP). (The address was delivered at the CAS-Annual Meeting, St. John's, Nfld., June, 1990).

Daria's most cherished awards are those for teaching medical students. At Queen's she was elected the best teacher almost every year, and at Western she received the "Best Teacher Award in (all) Basic Sciences" from the Students' Evaluation Course Committee and the Hippocratic Council (1978) and the "Hippocratic Council; S.F.C.S.C. Teaching Award (in all) Basic Sciences for 1983–1984". She particularly treasures "The Class of '62 Award", received on the Convocation Day of the Medical Faculty, June, 1982, because of its Terms of Reference: "To the Faculty Member, who, in the opinion of the Graduating Class, had contributed the most to the Medical School in the previous four years. Selection is by Graduating Class vote". She is the only female professor and the first pathologist ever so honored by her beloved students. When invited by another class to address, on behalf of the Faculty, the graduating students at Class Day Exercises on Convocation Day in 1981, she briefly summarized her feelings for students and the role they had played in her life: "We, your teachers, have been fortunate being associated with you, and we should be grateful to you..., for you keep us young at heart, and alert by your inquisitive minds, curiosity, dedication to a demanding profession, and yes — by your radiating youth. Thus, it should be we who thank you". Daria has been always strict and demanding with her students. Her aim has been to encourage enquiry and deductive thinking rather than memorizing data. "No matter how satisified you may be (with an answer), ask always: 'Why' — and 'How', — she would preach. She was always there, when needed, and often found herself on the students' side when they were not treated fairly by her colleagues.

Former residents, postdoctoral fellows, and graduate students have been another source of joy and pride. Whenever possible, a candidate applying for research was encouraged to pursue his/her own interests provided Daria felt sufficiently competent to supervise the work and her laboratories were equipped to support it. Even within her own main areas of endeavour, there has been considerable diversity of investigations by young people at any given time. Some work was based on human tissues, *e.g.*, "Cerebral changes in phenylketonuria"; "Duchenne muscular dystrophy"; "Hepatic (ultrastructural) changes in the Hurler disease"; "Evolution of cerebral and cerebellar changes in Sanfilippo disease"; "The heart in mucopolysaccharidoses"; "Leukemia of childhood"; "Placenta in 'toxemia of pregnancy' "; "The role of placental infections in the outcome of pregnancies"; "Elastogenesis in fetal human aorta"; "Immunoflorescent techiques in the study of human atherosclerotic lesions", and many others. Experimental work (also in collaboration with other colleagues) was used to test or clarify problems that arose in observations of human processes, and never was a means in itself. Most experiments related to arterial (specifically atherosclerotic) problems in man. For example, the work on spontaneous atherosclerotic aortic lesions of swine and on long-term different 'fat-feeding' was to test whether lesions that could have arisen from thrombi were altered by a (given) hyperlipidemia. The research on "Experimental hypertension in rats" was to test (by immunohistochemical methods) whether the thickening of coronary arteries in these animals resulted from permeability changes. An extension of this work was carried out in collaboration with Dr. S. Renaud (Montreal Heart Institute) using hyperlipidemic-hypertensive rats. "Experimental production of thrombi in rabbit aorta" was designed to examine sequentially the process of conversion of thrombi to atherosclerotic plaques with special reference to the involved cellular elements; this was relevant to Daria's own previous observations that, in man, the cells were smooth muscle cells, rather than fibroblasts. The work on "Experimental poststenotic dilatation of canine carotid and femoral arteries" was intended to test whether the arterial elastogenesis following a mild injury was identical with that in normal development.

Experimental work was also carried out to elucidate certain problems in nonvascular diseases. For example, long-term administration of pipecolic acid to rats was designed to test whether, at the ultrastructural level, tissue changes could be produced that resembled those observed in the newly discovered entity of hyperpipecolatemia in a child at CPRI. Similarly, high doses of various glycosaminoglycans were injected into animals and the tissues examined sequentially in the hope of unravelling the mechanisms operational in genetic mucopolysaccharidoses. The results of many of the investigations cited above constituted the basis of theses of Daria's graduate students.

In addition to young people, Daria on occasion accepted "visitors" at more senior levels; they would spend up to 6 months either engaging in experiments or in educational endeavours. They usually came from distant places, e.g. Prof. K. Koo came from South Korea (learning techniques of ferritin-conjugated antibodies for ultrastructural demonstration of albumin); Prof. C. Brandão from Brazil (studying modern methods and means of teaching pathology to medical students), and two WHO Fellows in Medicine came from China: Prof. M.-Y. Tang (Capital Hospital, Beijing, who studied the role of the placenta in the etiology and outcome of pathological pregnancies) and Prof. W.-Y. Huang (Cardiovascular Institute of the Chinese Academy of Sciences, Beijing, who evaluated, using ruthenium red, the status of glycosaminoglycans in human atherosclerotic lesions).

What was Daria's most exciting research experience? In general, in all investigative work the most exciting moment occurred when one could see completely new features of a cell, or an extracellular element. But this was only the beginning, as next, it was imperative to ask: "why was it there?"; for there must be always a reason in Nature's wisdom. The answers to these functional enquiries have been the culmination of excitement if they provided a reasonable basis for a new concept. For example, it was not as exciting to discover that the cells in every phase of human atherosclerotic lesions were smooth muscle cells rather than fibroblasts, as was universally believed, as it was to realize (in answer to one's own question: *why?*) that they were in that location for a very good reason. These cells were versatile; they not only had many of the (needed) functions of fibroblasts but other crucial properties (contractility; secretion of the precursor of elastase, *i.e.*, the enzyme required for remodelling of the arterial wall) and (unlike fibroblasts) "operated" in the absence of new thin-walled capillaries (which would rupture under high blood pressure aggravating the pathological process).

Heritable metabolic disorders were a continous source of excitement because these represented "experiments by Nature" and by careful observations (with a keen eye!) one hoped to link an altered structure to a metabolic aberration, or better even, interpret the latter in the light of the former. Daria was privileged to experience such elation when Dr. David Gatfield investigated at CPRI a group of patients with hyperornithinemia, hyperammonemia, and homocitrullinuria (HHH-syndrome) and found that no explanation for the hyperornithinemia as the level of activity of the specific enzyme ornithine-ketoacid transaminase was adequate. Since ornithine must enter the hepatic mitochondria to be metabolized to citrulline, an ultrastructural assessment of a liver biopsy was carried out on one patient. It disclosed considerably altered hepatic mitochondria with a thick "sieve-like" membrane interposed between the inner limiting membrane and the interior of the organelle. It was therefore proposed that, in the HHH-syndrome, ornithine is unable to enter the mitochondria because of the interference by this thick structure, and the defect thus lies in (transmembraneous) transport of ornithine. Later studies by others supported this contention.

While recognizing the importance of new tools and other disciplines (*e.g.*, biochemistry) in the unravelling of riddles in medical sciences, Daria believes, nevertheless, that careful observation of tissues in their natural state remains the most important key to opening an area to ("further") investigation. A recent example underscores this: it was a long and thorough study of tissues of two brothers obtained at necropsy, and of skin biopsies of their two (living) siblings that convinced her that the changes observed were inconsistent with any disease with which she has had experience. Thus, the field of scrutiny was narrowed by exclusion and ultimately led to the diagnosis of aspartylglucosaminuria. This is the first Canadian family so diagnosed.

Did Daria experience any frustrations? "They were largely man-made and do not deserve comments or remembering". Some experiences warrant perhaps a brief mention, because they affected "her people". She refers to the practice, still followed, of a "true" scientist: "Present your data first to an audience of your peers, take into account their constructive criticism, and then write a manuscript for publication". It was the experience of Daria and many of her students that few investigators in medical sciences respect that dictum, as most publications are already in print before oral presentations. Worse even: many of the new ideas so generously shared with colleagues at meetings but "unprotected" by a manuscript "in press", became the prey of pseudoscientists in the audience. On several occasions, before Daria and "her people" had time to "produce" a manuscript, the new ideas became somebody else's "property". She would offer to her disheartened young colleagues the "cheer-up motto": "He who has an original idea, will have more than one".

What are Daria's thoughts on her own contribution to science and how would she summarize these?

In terms of research that "induced" new concepts, was innovative in the world or novel in North America, the following is a summary of the work she carried out by herself or in collaboration with colleagues:

1. Identification of cells involved in every phase of atherosclerotic process as being smooth muscle cells (SMC) (1956) (rather than fibroblasts as believe until then), and their versatility: elaboration of all forms of connective tissues; lipid- accumulation; migration and multiplication.
2. The first demonstration (1972) that the arterial SMC secrete a precursor of elastase under normal and pathological conditions (hypertension; atherosclerosis). Years later these morphogenetic studies were confirmed by biochemical analysis.
3. The demonstration (1956) that thrombi play an important role in the inception and progression of atherosclerotic lesions; this old concept of von Rokitansky found support in the U.K. in the late forties of this century, but not in North America.
4. The first documentation in North America (1956) that altered endothelial permeability and its consequences (insudation of plasma constituents into the arterial intima) is an important factor in atherogenesis; this concept was proposed by and known to German pathologists.
5. The original formulation (1969) and extension (1970–1971) of the all-embracing concept of "injury and repair" in the pathogenesis of atherosclerosis.
6. The original formulation (1962) of the process of elastogenesis (formation of a "unit" consisting of two morphologically distinct components: a central core and surrounding microfibrils). Years later, this concept was substantiated by biochemical analysis.
7. The original definition (1965) of the smallest formed component

of extracellular connective tissues, *i.e.*, the microfibril, that "participates" in elastogenesis (vide #6), exists in a "free" form, and probably is involved in fibrillogenesis of collagen. Years later, the concept that microfibrils represent a distinct entity was substantiated by biochemical analysis (the possible relation of microfibrils to atherogenesis was suggested by others because they seem to aggregate platelets).

8. The original formulation (1970) of the mechanism of elastic fiber-calcification via microfibrils (vide #7).
9. The novel finding (1984–1987) of rudimentary cilia in SMCs and endothelium of experimental and human atherosclerotic lesions, and the proposal of their possible role in the proliferative process of atherosclerotic lesions.

In her second field of endeavour, *i.e.*, in research related to childhood diseases, there were also several "firsts" (*e.g.*, the first application of electron microscopy to the study of livers in genetic mucopolysaccharidoses (1962–1963) and other heritable metabolic disorders, and to the placenta) that yielded new observations and knowledge. However, Daria feels that her investigations contributed only in modest terms to entirely new concepts in this area. To these belong her observations on hepatic mitochondria in the HHH-syndrome (vide supra). If her work in that area had any tangible impact, it was by virtue of stimulating in Canada, and particularly in Ontario, interest (diagnosis; service and research) in, and concern for children with mental and physical disabilities. She has tried to pass on that concern to her medical students by introducing lectures on "mental retardation", and to her postgraduate fellows and graduate students (regardless of the subject of their investigative work) by requiring that they participate in some activities of the small department of pathology at CPRI under her direction. In her early years in London, she often accepted invitations to address the association of parents of these children, explaining the diseases and the importance of obtaining tissues at biopsies and autopsies for further investigations. Daria believes that, as a mother, she spoke to these parents in a language they could understand. Thus, the long prevailing reluctance in allowing autopsies to be performed on these children soon gave way to a desire to cooperate, and permissions for post mortem examination were then offered willingly.

In the area of (general) childhood diseases and obstetrical disorders affecting the fetus and the infant, Daria pioneered, both at Queen's and UWO, strong and ongoing collaborative efforts with colleagues in Paediatrics, Obstetrics, Neurology, and Biochemistry.

Daria's main impact upon her chosen field, *i.e.*, paediatric pathology, may possibly lie in her efforts (with other colleagues) to establish it as a distinct subspecialty. Quite deliberately, she undertook to demonstrate by educational means to her fellow (general) pathologists that the practice of paediatric pathology requires specialized knowledge. Thus, she organized (1964) a Refresher Course: "Examination of Placenta in Health and Disease" (and conducted it for several years) for the Annual Meeting of the Canadian Association of Pathologists; introduced it in 1967 at the Annual Meetings of the International Academy of Pathology (IAP) (organizing and directing it for a decade); and was invited to conduct it at the VII Latin-American Congress of Pathology, Buenos Aires, Argentina, 1969, and at other scientific meetings (St. Juan, Puerto Rico; Sydney, Australia; Beijing, China; Xian, China). In 1968 she also organized and introduced, at the annual meetings of USA/Canadian division of the IAP, a "Specialty Session on Paediatric Pathology" (with participating panelists) and conducted these for a decade. In 1974, Daria introduced, as a regular feature of the biennial International Congresses of the IAP, Slide Conferences and Symposia in Paediatric Pathology (the participants represented over a dozen countries from around the world), and in 1977 she established, as a permanent feature, similar programs at the (triennual) Congresses of the International Pediatric Association. It should be noted that all these programs still exist and are very popular.

After some two decades of documenting, scientifically and educationally, that Paediatric Pathology is a distinct sub-specialty, Daria moved on to the next step. Early in her presidency of the Society for Pediatric Pathology (SPP) (1983–1984), she began concerted efforts aiming at official recognition of the sub-specialty (examination and specialty-certification by the American Board of Pathology) by appointing a Special Committee of the SPP to work towards this end. She also used her contacts with personal friends and colleagues in (Adult) Pathology who served on that Board to present in person, on every possible occasion (!), arguments in favour of such move. "The coronation" of these efforts (with many dedicated colleagues in SPP) was announced recently: The first examination of candidates for certification of special qualification in Pediatric Pathology will be held by the American Board of Pathology on November 20, 1990.

What are the proudest achievements of Daria's long academic career? She is reluctant to label the following as her achievements, allowing, however, that she is proudest of two aspects of her professional life: 1. The rapport with and the affection of (most) medical students (of many generations), and 2. the achievements of her former residents, postgraduate fellows, and graduate students. She is quite proud that many have literally "outdistanced" her (*e.g.*, Dr. William Laign, the first Professor of Pathology at the first School of Medicine in Ghana (Accra), and later Dean of Medicine and Technology at the second Ghanian University in Kumasi; Dr. Kenneth Marshall, who became Assistant Dean of Medicine (Undergraduate Education) at McGill University; Dr. Jorge Las Heras, the first fully qualified Chilean Paediatric Pathologist, now Professor of Pathology at the University of Chile, Santiago; Dr. Roberta Bondar, a distinguished Neurologist on the Faculty of Medicine at McMaster University and the first Canadian female astronaut; Dr. Robert Maudsley, Professor of Obstetrics and Gynecology and currently

Associate Dean of Medicine, Queen's University). The above are only examples, as Daria is equally proud of the achievements of others who have been distinguished faculty members in medicine at Universities in Canada (*e.g.*, Dr. R. Armstrong, UWO; Drs. B. Lach, M. Orizaga, and G. Perkins, University of Ottawa; Dr. A. Trillo, Dalhousie University), the USA (Dr. J. Choi, University of Southern California, Los Angeles; Dr. J. Balis, University of South Florida, Tampa), and elsewhere (contact with quite a number of "her people" was lost, regrets Daria).

Daria especially wishes to acknowledge the contributions of the often "unsung heroes" who are, however, the backbone of day-to-day work, be it in the laboratories or in the preparation of grant applications, manuscripts, reviews, and teaching materials for intramural and extramural activities, *i.e.*, the technicians, research assistants, and secretaries. Of the technicians at Queen's, Mrs. Carolyn Martin, and of those who worked at Queen's and Western, Mr. Garry Woodcock, Mrs. Maria Oteruelo, and Mrs. Hermina Trillo deserve mention. Special acknowledgment, is owed to those who loyally devoted many years and are still working in her research laboratories (UWO and CPRI): Mr. Roger Dewar (23 years), Ms. Irena Wojewodzka (21 years), Mrs. Renate Feulgen (19 years), and Mrs. Susan Coulson (16 years). Similarly, without the help and expeditious assistance of Mrs. Sheila Collard, the Administrative Officer of the Department of Pathology at UWO (and her staff), many efforts (particularly recent ones) could not have been realized. Finally, Daria wishes to acknowledge the continuous support of Dr. Robert Goyer, currently the Chairman of the Department of Pathology at UWO.

Has Daria deliberately influenced anyone in the course of her professional life? She is firmly convinced that any influence by "design" is unethical because: "Every man must be left alone to make up freely his own mind". In only two instances was she consulted by young female physicians as to whether to enter a career in pathology. One was the only female final-year medical student at Queen's while Daria was a resident in pathology: "Ever since we began studying pathology, I have been fascinated by this discipline and wished to become a pathologist, but was discouraged by our instructors (pathology is not suitable for a female physician). Now I have seen you around as a resident. What do you advise?" Daria's answer to Dr. Eva Becker (who later became a paediatric pathologist) was: "If one does one's very best and quietly works hard and remains a lady, there should be no problem in becoming a pathologist". The answer to the second enquiry (by Dr. T. Wasan) was similar.

The undergraduate students and young colleagues under Daria's supervision often sought her advice. In each instance she took time to listen attentively, but instead of advising, she asked relevant questions. The answers were entered on paper in two columns: the "pros" and "cons". When completed, the advice-seeker was given the list and informed: "One can only assist an intelligent person in 'sorting out' the various aspects (of a given problem); the decision, however, must be yours"! Some students referred to these visits as "problem-solving sessions".

Does Daria have any regrets after over three decades in science and academia? No, not in terms of wishing to erase any aspect of that time, but in another context. One regret relates to professional life: in the "hustle and bustle" of that life, there was time to evaluate only a portion of the research carried out in her laboratories, and to publish even less. Daria regrets this because investigations on which some theses of her graduate students were based, while still original, remain unpublished. She adds: "Sadly, the real losers are the young people, who left for their careers assuming that I shall somehow find the time for their sake to write up the manuscripts for publication". She had, however, so little time!

Two regrets are of a purely personal nature. Daria would have liked to spend more time than was possible with her family. The other relates to the human need for continuous growth for which there was little time. She still hopes to enrol in courses in Anthropology, Philosophy, and Canadian history and pursue her few hobbies: music, singing in the church-choir, and long daily walks.

What are her hopes and concerns for the future of science in Canada and the place of young scientists in the future of this country? To avoid imprecise reference to the subjects in question it might be useful to turn briefly to the definition(s) of science (scientists), and also of research.

Science was defined initially as knowledge. Later, the definition acquired different shades of extended meaning. Science today may imply a systematic and formulated knowledge in reference to specific subjects, the pursuit of such knowledge, or the principles regulating such pursuit. Moreover, the definition includes a branch of knowledge (accumulated on a subject). Thus, pure science is knowledge depending upon deductions of self-evident truths (*e.g.*, mathematics) and natural or physical science is based largely on observations, experiments, and inductions (*e.g.*, biology; chemistry). Finally, an expert's skill (acquired by knowledge) also falls within the realm of science.

Research on the other hand, whereas adequately defined as "a careful search for, or enquiry into a subject", and "a course of critical investigation", for the purpose of the following arguments is best delineated as "an endeavour to discover facts on a (given) subject by scientific means".

In analyzing the above definitions, it becomes apparent that *knowledge* remains the common denominator, the basic ingredient, or the *sine qua non* of science and research alike. Knowledge on the other hand can be acquired only by continuous studying, by education. Thus, the key to knowledge is education. If a country is truly concerned about its future as a domain of civilized people, that country will invest its energies and resources in providing the best possible education to all citizens, so that each may be educated to his fullest potential. Science and scientists will develop naturally from that reservoir of educated (knowledgable) people.

Daria resents the notion that we, as a nation, should promote science and "produce" young scientists (doesn't this remind one of an assembly line in a factory?) because Canada must sustain her economic growth and be competitive in today's world. It is degrading to subordinate a nation, any nation, to the aims and means of accumulating material wealth only. A civilized nation with ongoing maximal effort in education, should possess an inexhaustible wealth vested in its people. It should be the decision of such (educated) people which areas of their accumulated "know-how" they wish to choose for their own endeavours. The other (objectionable) notion, promoted increasingly in Canada, is that this country "must be able to compete"! Why must a country, or for that matter people, compete? It was precisely that competitive aspect of the U.S.A. that (along with some other factors) convinced Daria in 1951 that Canada would be a better place for her to build a new life. And of late, hard competition has invaded Canada, too. Isn't it more humane (and "productive") that each person perform (in whatever capacity) as best he can and at his fullest potential rather than being motivated by "outdoing" someone else? It is the sum of people who follow their own creativity, and who are thus fulfilled, that represents the country's true wealth.

To achieve the above, one would require to begin with a revised format and intensity of education at the earliest exposure of children to learning, eliminate all forms of competition, and nurture in each child the desire to learn, curiosity, and hunger for knowledge. There would be no need to compete for the position of best, second-best, etc., in the class, as all students at all levels of education would be the best, *i.e.*, the best they could be by virtue of their own ability. Obsession with "learning" in order to be "the best", or to win a prize, has nothing to do with education. It is equivalent to training animals to perform certain "duties" or "tricks" in anticipation of being rewarded: by a pat, a lump of sugar, or a piece of meat! What is further disturbing is that such an ingrained attitude about "winning" a competition extends beyond school age (where perhaps in its origins, it was intended as a game), and permeates daily life (to think that this occurs here in Canada!).

If the educators of our beloved land do not soon bond together to promote strongly the motto: "learning for learning's sake"; and "science for the sake of science", one may be truly concerned about the future of Canada.

Sidney Van Den Bergh

(May 20, 1929 –)

Killam Laureate, 1990

Three of the first memories in my life share the same theme; awe at the vastness of the world. I remember (probably at age two) trying to empty a large river with a small pail, and suddenly realizing the limits on things possible to a small human being. Similarly I remember wanting to touch the moon, and discovering that its distance was so great that I could not do so - even if I used a long stick or climbed a tree! It is therefore perhaps not surprising that I've wanted to be an astronomer for as long as I can remember. At a very early age I taught myself to read by devouring books on astronomy, archeology, and geology. These subjects are still the main interests of my life, demonstrating that nature, not nurture, is the dominant driving force in the lives of many people.

My kindergarten teacher wanted to introduce me to a friend of hers, Sir James Jeans, who was one of the leading astronomers of his day. But I declined the invitation to meet him because I was afraid of being in the presence of such a famous man. (For much the same reason I avoided meeting Albert Einstein when I was an undergraduate student in Princeton in 1949).

My parents Sidney James van den Bergh, a successful businessman, and my mother Mieke (van den Berg) van den Bergh supported my early interest in science by buying me piles of books, a telescope and a microscope. During my teenage years Dad did, however, urge me quite strongly to keep astronomy as a hobby and do something more practical for a living.

Travel has always been an important part of both my personal and professional life. I was born in Wassenaar, Holland on May 20, 1929 and lived briefly in Germany before moving to England in 1930. At the onset of the war in 1939 we moved back to Holland experiencing the horrors of the German occupation. After studying for a year (1947-48) at Leiden University I managed to get a scholarship to Princeton, where I obtained an A.B. in 1950. After that I attended the Ohio State University (M.Sc. 1952), and the University of Göttingen (Dr.rer.nat. 1956). My first position was as an assistant professor of physics and astronomy at the Ohio State University. A better job offer brought me to the University of Toronto where I taught astronomy from 1958 to 1977. After that I moved to Victoria where I was Director of the Dominion Astrophysical Observatory from 1977 to 1986.

During my studies I was privileged to hear lectures by many famous scientists. I must, however, confess that none of these really influenced me, or my scientific thinking, very much. Usually I preferred to read independently in the astronomical literature and then would start to think about the things that I read. Discrepancies between theory and observation usually turned out to be particularly promising starting points. The three astronomers whose writings influenced me the most were Edwin Hubble, Walter Baade and Allan Sandage. Among the teachers whose classes I attended the most outstanding was Professor Marcel Minnaert of Utrecht. It is hard to believe that this fine and gentle man, who seemed to love every electron that ever radiated a photon, was once sentenced to death for allegedly collaborating with the Germans in his native Belgium during World War I. My deep loathing for the death penalty probably goes back to the cold winter mornings of 1943 and 1944 when the German firing squads could be faintly heard from my home.

While I was working on my doctorate in Göttingen the first prints of the Palomar Sky Survey arrived at the Observatory. I was utterly fascinated by this, the deepest and most complete survey of the observable universe, that had ever been made. After poring over these splended photographs for many hours I almost felt as if individual galaxies and star clusters started talking to me. By being very quiet and "listening" to them I found out that most galaxies with low surface brightnesses were dwarfs. This idea ultimately led to the DDO Catalog of Dwarf Galaxies. It also soon became obvious that the morphology of spiral galaxies correlated with their luminosities. Pretty galaxies with well-developed spiral structure turned out to be luminous, whereas "ugly" spirals with scraggly spiral arms were found to be intrinsically faint. Inspection of galaxies in rich clusters showed that these objects differed, in rather subtle ways, from similar objects in the field. The importance of such differences were not really understood until about twenty years later.

Inspection of the prints of the Palomar Sky Survey also made it possible to catalogue Galactic supernova remnants and to tell the difference between old and young Galactic star clusters. By looking at the morphology of dust clouds in the plane of the Milky Way I convinced myself (but few of my colleagues!) that one could read the history of star formation in and around these objects by recognizing worm-like "scars" produced by expanding HII emission spheres around luminous hot stars that formed and died in their vicinity.

I find that I can usually stay intensely interested in a particular scientific problem for only a few months. Then I write up my results and move on to another area of astronomy. I might, however, return to the same subject again after one or two years. (A single attempt to spend three years of my life writing a monograph on a single topic ended up as a complete disaster!) The main topics that I have been interested in during my scientific career are: the structure and evolution of galaxies, the extragalactic distance scale, supernovae, star clusters, and variable stars. Very recently I have combined my interests in astronomy and geology to write a paper on the extinction of the dinosaurs. At first sight it might appear that study of the extinction of the dinosaurs, that took place 66 million years ago, is one of the most academic and useless fields of scientific endeavour. However, it is becoming increasingly clear that this global catastrophe probably involved "nuclear winter", global deforestation, acid rain, disappearance of the ozone layer, and the greenhouse effect. These are all problems that are now recognized to be of vital significance to the continued existence of humanity on this small planet in the inner solar system.

Most of one's life as a scientist is devoted to teaching, to enjoyable but more or less routine work, interactions with scientific colleagues, and seemingly endless airplane trips. Occasionally one is, however, privileged to discover something new. Although my own discoveries have all been relatively minor ones they have given me pleasure. There is something really exciting about the feeling that you're the only person in the world who knows a particular thing. The first time this happened to me I remember thinking that I had to be extra careful crossing a busy street, so that the small secret of nature that I had discovered would not be lost if I got run over by a car! (On growing older one realizes that a shiny pebble dropped by one person is sure to be found by another). The exhilaration of chancing upon a new thing or idea is, in some ways, similar to the feeling I experienced many years ago when I made two first ascents of (not very high!) mountains in the Peruvian Andes. There is always something very special about exploring i.e. being somewhere where no-one else has ever been before. This, together with the company of scientific colleagues who are also sharing the long road of discovery and search for "truth", is one of the greatest joys of a career in science.